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NOTES AND COMMENTS

Bubonic Plague in Vienna

The occurrence of fatal cases of plague in connection with the Bacteriological Laboratory at the General Hospital in Vienna has attracted much comment from the press, and is not without its lessons. It may appear surprising to some that such incidents are not of more frequent occurrence. As a matter of fact they are very rare, and this for two reasons. The majority of pathogenic organisms soon lose much of their virulence when cultivated for any length of time outside the body; some become harmless in a few days, others not for weeks or months, while there are bacteria which seem to retain their pathogenic powers almost indefinitely. In most cases virulence may be restored by suitable passage through the animal body. The chief reason, however, for the rarity of accidents lies in the routine precautions taken in the laboratory when dealing with pathogenic organisms. Such precautions are the first lessons impressed upon the student; for they are necessary, not only as a safeguard to the experimenter, but in order to preserve the cultures themselves from contamination. Carelessness shows itself at once in impurity arising in the cultures, and although the carelessness which leads to this is not necessarily the same carelessness which contaminates the worker's hands, yet both spring from the same cause, and one is rarely present without the other. Cultivations are carried out on moist media, and micro-organisms growing on such media do not escape into the air and do not seem to constitute any source of danger by inhalation. In the desiccated condition this is not so, but cultures are usually discarded and destroyed before they become dried up, and, moreover, drying is fatal to many kinds of bacteria. In all laboratories the beginner acquires, or ought to acquire, the technique necessary for the protection of himself and his cultures by practice upon harmless organisms. Once acquired, it becomes in time practically a reflex action, and the fear of infection is scarcely present to the mind.

Nevertheless there will always be reckless persons, and accidents will at times occur. Some organisms are especially virulent and dangerous to work with, for instance, the bacillus of glanders. Even

typhoid fever is perhaps at times contracted in the laboratory, and one fatal case of cholera has been definitely traced to this source. Such instances are, however, so rare as to be of historic interest. Laboratory infection is, in fact, a risk almost infinitesimal in comparison with the risks run in the *post-mortem* room or at the bedside, or even in a crowded omnibus. It is the dog in the road who bites you, not the dog you keep chained up in a cage. The recent case of plague at Vienna appears to have been due to carelessness on the part of a drunken laboratory attendant, if, at least, we may trust the accounts which have appeared in the daily press. The lamentable deaths of the physician and nurse in attendance were due to direct case to case infection. The drastic measures which were taken by the authorities appear to have been completely successful in checking the spread of the disease—a gratifying tribute to the efficacy of modern sanitary precautions. The warning against carelessness in dealing with pathogenic organisms is obvious and perhaps timely, but we are far from agreeing with those who, in a moment of panic, would raise an outcry against their study in the laboratory. The good that must result and has resulted from these investigations far outweighs such rare calamities as the recent plague cases in Vienna; the occasional warning should only emphasize the need of prudence and caution in research.

The Borings at Funafuti

Two instalments of news have recently come to hand concerning the coral-boring expeditions, whose plans were related in our July number (vol. xiii. pp. 70, 71). Commander Sturdee's bold experiment of boring in the centre of the lagoon from the bows of his ship (H.M.S. "Porpoise") was a remarkable success. He moored his vessel so taut that it was possible to work the hydraulic boring pipes without risk of their bending or breaking. Mr G. H. Halligan, who was in immediate charge of the boring plant, reports that the first bore reached a depth of 144 feet, the total depth of the bore being 245 feet below the water level of the lagoon, the depth of water to the bottom of the lagoon being 101 feet. The first 80 feet below the bottom of the lagoon passed through sand, composed of segments of *Halimeda* (a seaweed which secretes a jointed stem of lime), and of fragments of shells. This gradually changed into a coral gravel, the fragments being at first small but getting larger at the deeper levels. At the bottom a mass of very hard coral rock was met with, and had to be drilled with a steel chisel. An attempt was then made to enlarge the hole with an underreamer; but in the process the bore-hole became choked with coral gravel. Efforts to drive the boring-pipes through this with an iron 'monkey' were unsuccessful, and the hole was abandoned. Another

bore was then started 90 feet nearer the centre of the lagoon, in the same depth of water. The time allotted did not permit the bore to reach a greater depth than 113 feet, and the material was similar to that in the other bore.

The deepening of the old bore, discontinued last year at a depth of 698 feet, on the main island of Funafuti, at first proceeded slowly, having been delayed by a temporary failure of water. The party were landed by the London Missionary Society's steamer, "John Williams," on June 20th last. As was anticipated, little difficulty was experienced in re-driving the lining pipes into the old bore, and washing out the sand and rubble which had choked the bore-hole. Pipes were laid from the site of the old bore to some small water-holes from which a supply of fresh water was obtained for the boiler. By July 25th, the re-lining and cleaning of the old bore having been successfully accomplished, boring was resumed, and when the "Porpoise" finally left on September 6, a depth of 987 feet had been reached. The bore last year terminated in soft dolomite limestone at 698 feet, but below this is a hard rock, so hard that the portion of the bore-hole which penetrates it no longer needs to be lined with iron pipes, a condition which facilitates the work of boring.

Mr A. E. Finckh reports that this hard rock is largely composed of corals and shells. At a depth of 840 feet on the ocean face of the reef there is a strongly marked shelf, as shown by the soundings of Captain A. Mostyn Field of H.M.S. "Penguin," and it is considered that this shelf, at the 140 fathoms level, marks the downward limit of the coral formation. The fact that coral rock occurs far below this depth strongly suggests that subsidence must have taken place.

Mr Finckh has conducted successful experiments on the rate of growth of the various reef-forming animals and plants. In conjunction with Mr Halligan he has accomplished some satisfactory exploration of the deep-sea reefs. They have also levelled accurately several lines of section across the atoll, and fixed large and permanent datum-marks for the benefit of future observers. These will show whether the atoll is undergoing elevation or subsidence, and whether it is growing seawards or not. The boring was to continue and Mr Finckh was to go on with his observations until the middle of November. The diamonds, however, had almost run out.

We are indebted for this information to our correspondent, Mr C. Hedley. The results, so far as one can judge, are compatible with Darwin's theory; but we must wait until the cores, already brought to Sydney by Mr Halligan and Foreman Symons have been subjected to thorough microscopic and chemical examination. Until this has been done no opinion can be offered as to the conclusions, but we

can all unite in praising the perseverance of our Sydney friends and the bold conception of Commander Sturdee, and in congratulating them on their truly remarkable success.

The Babel of Terminology

"The Witness of Science to Linguistic Anarchy" is the startling title of a sixty-four page pamphlet compiled by Lady Welby and printed for private circulation. By collecting from different scientific writings a number of passages in which technical terms are used in different senses, or in which such varying use is criticised, she aims at showing: first, that the language employed by scientific authors lacks that very consistency and precision which we have a right to expect; secondly, that concerted action ought to be taken with the view of securing a general consensus in usage. The extracts, chiefly from *Nature*, *Science*, and *Natural Science*, are sorted under the headings: General, Physics, Biology, and [Taxonomic] Nomenclature. These quotations, she believes, to constitute "evidence of an almost incredible state of things in the scientific world."

Incredible though it may appear to "a humble layman," as Lady Welby terms herself, the situation is painfully familiar to every worker in science; and there must be many who agree with us that it is like to go on to the end of the chapter, despite every well-meant effort towards reform. For, what are the causes of this lawlessness? They are mainly two, and those two are very distinct in kind. The first, in the words of a great master of language, is "pure ignorance, madam!" Setting aside the half-educated dabblers in science, how many of us there are who are sometimes impelled just a little out of our depth. *Ne sutor ultra crepidam*, is admirable advice more easily given than followed. This cause is one for which remedies are conceivable. We might, for instance, make publication a penal offence, only permissible to those who had passed the severest examination, and we might burn in the market-place all writings unlicensed by an international censorship which should have absolute control over every new term proposed. We do not say that these remedies are practicable, and we fancy that even Lady Welby will not admit them to be desirable; but the facts of history permit us to imagine them. The second cause is one that lies at the heart of the whole matter, and must persist so long as scientific investigation continues; it is, in fact, the advance of science itself, and for it the only conceivable cure is absolute stagnation, which is no cure at all. The widening of knowledge renders it impossible for a term or a name to have precisely the same connotation to-day as it had twenty years ago. A new species may involve the rediagnosis of its genus; a fresh

observation may necessitate a modified definition. A term that was in use when knowledge was vague and general must be restricted or abandoned as knowledge becomes more detailed and accurate. One man will restrict the meaning of the old word, another will propose a new word; and the number of new terms proposed will vary in proportion to the number of investigators. Each approaches the problem from a different standpoint; and who shall decide which conception is the correct one, which term is the best?

Scientific terminology is, and always must be, in a state of flux. Scientific language is, in this respect, just like any other language. And its evolution is subject to the same laws as govern, not merely all language, but most other mutable things. Whatever those laws may be, we cannot admit with Lady Welby "that in all other directions the first condition of human advance has been the implicit conviction that such advance was possible,—and was imperative." Take Lady Welby's own instance, the alphabet. Which alphabet? In Europe alone there are at least seven alphabets in use, and each of those has many ways of writing its constituent letters. In our own islands the same letters express different sounds according as one is writing Welsh, Gaelic, or English. Even here uniformity is a delusion: in language it is a vain hope. Whether we wish it or no, change will take place, new terms will be proposed, and the fittest will survive.

We would not appear to undervalue the labours of Lady Welby. Those who care to write to her at Denton Manor, Grantham, for a copy of her pamphlet will find it instructive reading. If she will continue her task, and will bring together the passages in which modern scientific terms were originally proposed or subsequently modified, she will render greater service to the cause she has at heart than by the republication of mere querulous criticism. And if towards the second edition that Lady Welby promises we might add a final suggestion, we would write across the blank pages, so thoughtfully provided, the one word, "Index."

The Authorship of Illustrations

In our review of Professor W. W. Watts' "Geology for Beginners" (p. 422) attention is drawn to the figures of fossils said (with some inaccuracy) to have been "prepared for" Zittel's "Grundzüge," and to the fact that no reference is made to the original source of those figures, many of which are certainly not to be ascribed to Professor Von Zittel himself, though the student might be led to suppose that they were. Such are Walcott's restoration of a section across *Calymene senaria*, here wrongly assigned to *Triarthrus becki*; Holm's restoration of *Olenellus kjerulfi*; Owen's picture of the pearly nautilus

in its bisected shell; Henry Woodward's restoration of *Pterygotus anglicus*. We think it right to remark specially on this feature, because it is an instance of a habit far too common with writers of text-books. Let us look at a few other recent cases. Professor W. B. Scott, in his "Introduction to Geology," favourably noticed by us last April, has copied Dr Traquair's first (1888) restoration of *Pterichthys* from Bashford Dean's "Fishes, Recent and Fossil," and has marked it "From Dean after Smith Woodward." The same legend is attached to Dean's own faulty attempt at a restoration of *Dipterus*, and we can only hope that Smith Woodward feels complimented. Parker & Haswell's "Text-book of Zoology" teems with similar errors. Ray Lankester's figures of the head shield of *Cephalaspis* and Traquair's restorations of *Pterichthys* are marked "From the Brit. Mus. Cat. of Fossil Fishes," although the writer of that Catalogue was careful in both cases to acknowledge the authorship of the figures. Similarly Traquair's restorations of *Palaeoniscus macropomus* and *Platysomus striatus* are marked "From Nicholson and Lydekker," in spite of those authors having expressly noted the figures as "after Traquair." So, too, a restored outline of the bones of the shoulder-girdle of *Plesiosaurus* is said to be "after Zittel," who all in vain had taken care to state that it was "nach Owen." Then comes Mr Beddard with his book on birds, reviewed by us this month, and assigns to Andrews the authorship of Ameghino's figures of the skull and pelvis of *Phororhacos inflatus*, simply because Andrews copied those figures (with due acknowledgment) in his paper in *The Ibis*. Examples crowd to our hand, especially in books by lesser writers, but we have only room for one more scapegoat. Last year Professor A. Issel published a "Compendio di Geologia." In this Huxley's old (1862) restoration of *Holoptychius* is attributed to "Traquair," while Pander's ancient restoration of *Asterolepis ornata* is marked "Pterichthys (Traquair)." As this was published by Pander in 1858, we should imagine that Dr Traquair was then a schoolboy.

Now is not this a parlous state of affairs, evincing what the costermonger described as "a very careless handling of the truth" by a class of men whose studies are supposed to lead them to a special reverence for truth and accuracy? To depict an elaborate dissection or to construct a restored figure of an extinct animal, is just as much an embodiment of the results of original research as pages of written description. The attribution of such figures to the last text-book writer that has happened to copy them is an act of scandalous injustice to those to whose patient research the said figures are due. The purchase of *clichés* or of permission to photograph woodcuts is a purely commercial affair, and it matters not at all from whose work a restoration was last copied. In any case, if

an advertisement has to be given, it need not be in the form of an untruth. The student wishes, we presume, to be directed to the original source of the figure and not to be sent on a wild-goose-chase from text-book to text-book. On the latter principle the whole of "Hamlet" might be attributed to writers in the daily press.

We can put the point quite clearly to the gentlemen referred to above. How would they like to see the results of their own original researches placed to the credit of any author that chanced to quote them? Or how would they like to have the obsolete ideas of older authors fathered on them? At the least they would denounce such action as a gross injustice, and they might say that it contravened the most elementary notions of morality.

Photography in National Museums

We have so often urged the advantage of having a permanent photographic establishment connected with our museums, that we welcome the address recently delivered to the Royal Photographic Society by its President. All the more do we welcome it, seeing that the President is the Earl of Crawford, who, as one of the Trustees of the British Museum, speaks with knowledge of the need and of the practicability of supplying it without any additional cost to the nation. Berlin and Munich, he pointed out, have shown that a photographic department can issue the finest work, can be of incalculable value to institutions in all parts of the world, and yet can be self-supporting. He pleaded forcibly that some such establishment should be attached to the British Museum.

In this connection it is satisfactory to learn that at least one small step in the desired direction is being taken at the Natural History Museum. A studio for the use of photographers is being built, and when this is completed it will be possible for investigators to photograph the specimens in the collection under better conditions of lighting and of quiet than are now attainable. We do not, however, learn that it is contemplated to attach a photographer to the establishment, or even to train an attendant as operator. The chief use to which the studio will be put will be the photography of specimens for the illustration of the Museum catalogues. Those who wish to utilise the accommodation for other scientific purposes will, whether they be connected with the Museum or no, still have to import their own apparatus and their own photographer at an expense which weighs heavily upon most students of natural history, and seriously hinders the proper illustration of scientific papers. However, the wedge is being slowly driven in; we must be grateful, and we live in hope.

The Frank Buckland Collection

Among other matters that cannot but prove interesting reading to the authorities at South Kensington Museum, the Report of the Select Committee recently issued contains the following paragraph:—

"We recommend that the Museum of Fish Culture should be abolished. Previous recommendations to this effect have been made. The Secretary and the Director both agree that it should be removed, and it has already been offered to two public bodies, being rejected by both. The fact is that this collection is dangerous owing to the large amount of alcohol in which the fish are stored; it is obsolete, not having been revised or increased for several years; and it does not carry out its obligations under the testamentary conditions of Professor Buckland's will. It occupies a good deal of space. Opinion being unanimous, we hope that this collection may disappear without delay."

The state of things is no doubt disgraceful, but the remedy proposed seemed too severe to many naturalists, and among others to the Piscatorial Society, which appointed a committee, consisting of Dr C. S. Patterson, Mr G. J. Chatterton, and Mr C. E. Walker, to investigate the matter. The following report was unanimously adopted by the Society:—

"The committee inspected the collection, which they found in a deplorable condition, and quite inadequate to carry out the testator's intentions, evidently owing to absolute neglect since it was taken over. There being no catalogue it is impossible to determine how much of the original collection still exists. The purchased additions apparently consist of something less than two dozen specimens, the majority of which have no direct bearing upon British fish industries. A large amount of the space allotted to the exhibit is taken up by objects which, however interesting in themselves, have no connection with either fish or fisheries. Your committee fully endorse the opinion of the Select Committee of the House of Commons as to the danger arising from the specimens preserved in spirits, as the building is entirely unsuited for the storage of such exhibits, but fail to see the point of the objection as regards the Buckland bequest, inasmuch as the majority of the fish in alcohol belong to the Day collection, which is not in any way an industrial exhibit, and should be placed in the Natural History Museum. As regards the testator's intention to provide a consulting and reference room for his fellow-countrymen, whether interested in sea or river fisheries, your committee are of opinion that such an educational centre is urgently needed, and that the collection in question, although inadequate through neglect, is capable of being brought up to date and of taking the place contemplated for it by the donor. Subject to Mrs Buckland's life interest a sum of £5000 was bequeathed to the Director and Assistant-Director of the South Kensington Museum in trust for the British nation to provide lectures on fish culture in connection with this unique series of specimens. Your committee, however, have

failed to ascertain what has been done with this money. All that they know is that no such lectureship exists, despite the statement of Mr George Bompas in his 'Life of Frank Buckland,' published in 1885, that after the death of Mrs Buckland '£5000 was given to found a lectureship.' Your committee have read with regret the recommendation of the Select Committee . . . [quoted above].

"In face of this recommendation the matter is urgent, and your committee are of opinion that steps should be taken at once to avert such a calamity as the extinction of this collection would prove to be. With this object they advise that the whole of the facts of the case be made public through the instrumentality of the Press, and, if necessary, by the question being raised in the House of Commons."

We agree that the destruction of the collection and the ignoring of Frank Buckland's intentions would be matter for great regret. Could not the Marine Biological Association take the matter up?

New Museum Buildings at Liverpool

On July 1, Sir William Bower Forwood, Chairman of the Library, Museum, and Arts Committee of the City Council, laid the foundation stone of the new Technical School and the extension of the Museum Buildings at Liverpool. The present museum buildings stand on a plateau sloping abruptly towards the west. By excavating the slope, which consists of Permian rock, down to the level of Byrom Street, sufficient accommodation, three storeys in height, will be provided for the Technical Schools, while the Museum galleries will be carried forward on their present level over the Schools. The architect of the new building is Mr E. W. Mountford of London. The extension of the Museum will be 90 feet above the level of Byrom Street, and will measure from N. to S. 162 feet, and from E. to W. 190 feet. The galleries, of horseshoe shape, will be continuous with those in the existing building, and will not be divided in any part of their course by walls or partitions. They will be 420 feet long and 33 feet wide; the lower, to contain the invertebrates, will be 19 feet high and lighted from the side; the upper, to contain the vertebrates, will be 27 feet high and lighted from the roof. There will be new and well-appointed laboratories for the Director and his assistants, as well as new administrative offices. The buildings will be of brick, faced with Stancliffe stone from Darley Dale, Derbyshire. They will be ventilated and heated by 4 miles of 3 in. pipes, discharging into every room purified and warmed air to the amount of eight million cubic feet per hour. The stairs are to be of stone, the floors of concrete, and the roof chiefly of steel. These buildings will be, next to St George's Hall, the largest in the city and probably the finest museum buildings in the United Kingdom outside London. It is

most cheering to see the attention now being paid to scientific and technical education and to intellectual culture in the great commercial city of Liverpool.

The Reduction of the Teeth among Mammals

The professors and lecturers in our Agricultural and Veterinary Colleges have many opportunities of making substantial contributions to biology. They deal with a series of problems which the student of organisms uninfluenced by artificial surroundings can rarely hope to find within his range. They observe changes in the structure and function of organs which they are able to correlate with known factors in the process of domestication or cultivation of the animals or plants they happen to study. They also have facilities for embryological research among the vertebrates, far exceeding those obtainable under any other circumstances. We who are interested in the purely theoretical questions of biology thus welcome with peculiar gratification any contribution from a naturalist who has taken full advantage of these favourable conditions, and enlivened the dull routine of teaching with comparatively broad generalisations.

One such contribution is contained in the seventy-ninth annual *Programm* of the Royal Wurtemberg Agricultural Academy at Hohenheim, dated 1897, but received from the author, Professor W. Branco, a few weeks ago. Professor Branco is the distinguished palaeontologist who succeeded Quenstedt in the University of Tübingen, but was unfortunately compelled by ill-health to relinquish the duties of the professorship after too brief service. Having now happily recovered, he devotes his energies to the Academy of Hohenheim, and his unusually wide sympathies in biology still stand him in good stead. A short time ago he published a description of some peculiar teeth, almost human in shape, from the Upper Eocene or Lower Miocene 'Bohnerz' of Wurtemberg (*Jahresh. Vereins für vaterl. Naturk. Württemb.*, 1898, pp. 1-140, pls. i.-iii.). He now follows this memoir by the present contribution, which discusses the nature and origin of the reduction of the dentition among mammals in general.

After some preliminary considerations and a broad outline of the facts, Prof. Branco applies the knowledge he has obtained in the course of his professorial duties. He points out that one principal cause of the reduction of the teeth is the shortening of the jaws. Among domesticated animals this shortening is shown to be due to at least two causes. It happens when the food requires comparatively little mastication; animals of any particular race fed upon soft food produce short-faced descendants, while others of the same race continually fed upon hard food always retain longer jaws and

face, also more slender limbs. Continual in-breeding tends to keep the jaws and face elongated and the limbs slender; while the frequent accession of new blood has precisely the reverse effect. In connection with this Prof. Branco ingeniously remarks, that in early Tertiary times, when there were much fewer mammals than in later times, in-breeding must have been comparatively common and may thus account for the universal long jaws and numerous teeth characteristic of all genera of the period.

A second cause of the reduction of the mammalian dentition is the preponderating growth of one or more of its components; such as the excessive development of the canines in *Sus*, and of the last molar in *Phacochoerus*.

As already long recognised, teeth also disappear when their function is lost. Hence the loss of the upper incisors in ruminants and most of the incisors in elephants, when the tongue and the trunk respectively usurp their functions. Hence also the loss of canines when effective weapons in the form of horns appear.

Finally, it follows from these considerations that changes in the mode of life and feeding have always been most potent factors, not only in modifying the individual teeth, but also in tending towards their reduction in number and the preponderating development of a few.

A New Peripatus

A good deal was said about the Peripatidae in the pages of *Natural Science* in connection with the publication of the fifth volume of the Cambridge Natural History, and in the series of short articles giving the opinions of various experts on the classification and constitution of the Arthropoda.[1] Since that time the literature of this interesting family has been enriched by several papers, by far the most important of which is Dr Willey's memoir on the species he procured in New Britain (see *Natural Science*, vol. xiii. p. 280). New Britain is an island lying off the east coast of Papua and forming part of the Austro-Malaysian sub-region of the Australian Region of Sclater and Wallace. Hitherto no member of this group had been discovered in this area, although several species have long been known from the adjacent sub-regions of Australia and New Zealand. One species too has been recorded from Sumatra; but some authorities, including Dr Willey, seriously doubt the accuracy of this locality on the grounds that this alleged Sumatran species is apparently generically identical with the Neotropical members of the family. Other zoologists, on the contrary, not unmindful of such facts as the distribution of the existing species of tapirs in the large Malay islands and in tropical America, are not quite so sceptical and see

[1] Vol. viii. pp. 122, 215, and 285; vol. x. pp. 97 and 264.

nothing particularly unusual in the existence of close relationship between a Sumatran and Brazilian species. Now if Dr Willey's *Peripatus* had proved to belong to the same type as the Sumatran and American species, then presumptive evidence in favour of the correctness of the locality assigned to *P. sumatranus*, would have been supplied. This, however, is not the case. Nor is it a particularly surprising fact, considering the great faunistic differences that obtain between Sumatra and New Britain with regard to many groups of animals, especially those, like the Peripatidae, with very limited means of dispersal. More surprising is it on zoogeographical grounds that the New Britain species also presents no near affinity with the species that are met with in Australia and New Zealand, seeing that the latter are congeneric. So, too, is it equally distinct from the last remaining type, namely, that which inhabits S. Africa. The great interest attaching to this species lies in fact in the circumstance that it occupies an isolated position and is distinguishable from the rest of its allies in exactly the same way that they are distinguishable from each other, that is to say in external structural characters, in details of internal anatomy and in the mode of development of the embryo. And since the other previously known types had been designated by generic names, there was no other course open to Dr Willey than to assign a name to his new species. Unfortunately, he prefers, for unstated, but no doubt excellent reasons so far as they go, to regard the sections of Peripatidae as merely of sub-generic importance—unfortunately, because in nine cases out of ten, such titles always assume the higher rank, and no doubt *Paraperipatus* will follow its destiny. The result is that this new form rejoices in the sixteen-syllabled title of *Peripatus* (*Paraperipatus*) *novaebritanniae*. Regarding these divisions for the moment as genera, we now have the following: *Peripatus*, Neotropical Region and Sumatra; *Peripatopsis*, S. Africa; *Peripatoides*, Australia and New Zealand; and *Paraperipatus*, New Britain. The characters in which these genera resemble and differ from each other are usefully summarised in tabular form on p. 37 of Dr Willey's memoir.

A New Palaeozoic Sponge

Dr J. M. Clarke of Albany has sent us a paper contributed by him to the *American Geologist* (vol. xx. pp. 387-392, pl. xxiii., Dec. 1897), on "A Sphinctozoan Calcisponge from the Upper Carboniferous of Eastern Nebraska." The new sponge, to which Dr Clarke has given the name *Amblysiphonella prosseri*, is nearly cylindrical in form and about 100 mm. (4 inches) in length. It is built up of a vertical series of chambers or segments, and a central cloacal tube extends throughout its length. The interior wall, the transverse septa roofing the chambers, and the cloacal walls, are

directly traversed by canals. The chambers are also irregularly divided by thin, apparently non-perforate, partitions. The walls are now of crystalline calcite of secondary origin and nothing is known of their original characters.

This sponge is closely allied to forms from the *Productus* limestone of the Salt Range, India, and from the province of Asturias, Spain. So far as mode of growth is concerned, these Carboniferous Calcisponges resemble the similarly segmented sponge genera *Barroisia*, *Thalamopora*, *Tremacystia*, &c., from the Lower and Upper Greensand, but no true comparison is possible until the minute nature of the wall in the palaeozoic forms has been ascertained. If this should prove to be spicular in character, there can hardly be any further doubt of their relationship to the Cretaceous genera referred to. These latter have recently been placed with the Sycons by Dr Rauff, but their true systematic position is not yet fully established. It should be remembered that the segmented mode of growth is not limited to the Pharetron Calcisponges; it is also well shown in the Jurassic Hexactinellid genus, *Casearia* of Quenstedt.

Botany and Agriculture

We have received several publications which are of interest to the agriculturist abroad. From New South Wales comes a "Manual of the Grasses" of the colony, issued by the Government botanist, Mr J. H. Maiden. It is a praiseworthy attempt to bring the native grasses before the farmer and botanist, not only for purposes of identification, but as a guide for their cultivation and improvement. New South Wales has, according to present knowledge, about 200 indigenous species included in 56 genera, and of each of these the author gives an adequate botanical description (in English), with references to figures previously published, and in addition any information he has been able to gather on the value as fodder and other uses. There are also notes on habitat and geographical range in the Continent. Of some of the species useful figures, including habit and floral dissections, are given.

Mr Maiden also sends a pamphlet on the vegetation of Lord Howe Island, to which he paid a short visit at the beginning of the year. His collections have resulted in several additions to the flora of this little island, which, as at present known, includes 217 indigenous species of flowering plants and ferns, with 20 introduced ones. The chief difficulty with which the farmer has to contend is the wind; it is said to be no uncommon thing for it to blow strongly for three months at a stretch. The wind-break question is therefore an important one, and every patch of cultivation is protected by belts of indigenous or planted trees.

A contribution towards a Flora of Mount Kosciusko, also by Mr Maiden, is an account of an expedition of a few days to the highest mountain in Australia, 7328 feet or more above sea-level. It includes a list of the plants found at different altitudes.

The U.S. Department of Agriculture sends a couple of pamphlets issued by Dr Hart Merriam, chief of the Biological Survey. One, entitled "Life Zones and Crop Zones of the United States," is invaluable to the farmer. It embodies the results of ten years' study of geographical distribution applied to practical agriculture. North America is divisible into seven transcontinental belts or life-zones and a much larger number of minor areas, each of which, up to the northern limit of profitable agriculture, are adapted to the needs of particular kinds or varieties of cultivated crops. A coloured map shows the distribution and limits of the zones, while the text contains lists of the varieties of cereals, fruits and other crops, which may be profitably grown in each area. Dr Merriam expresses the hope that his report will serve to emphasize the extreme wastefulness of indiscriminate experimentation, by which hundreds of thousands of dollars are thrown away each year in futile attempts to make crops grow in areas totally unfitted for their cultivation. It also suggests alternatives where, owing to increased competition or diminished demands, the farmer receives an inadequate return for his labour. For instance, in northern New York and elsewhere, where dairying is an almost exclusive but unprofitable industry, the land is shown to be adapted for sugar-beet, or several excellent varieties of wheat and other crops to which little or no attention is now given. The second bulletin, by Prof. C. S. Plumb, is a special instance on the same lines. It deals with the geographic distribution of cereals, and indicates what varieties may be grown with profit in each area.

The Rhone Beavers

These unfortunate animals decrease each year by reason of chase and flood. Mr Galien Mingaud writes in the *Revue Scientifique* that no less than nine of these rodents were captured during 1897 in the delta of the Camargue, at the junction of the Rhone with the Gardon. The beaver goes up this latter river as far as Pont-du-Gard. Two years ago Mr Mingaud addressed a note to public officers and to naturalists at large, asking for the protection and preservation of this animal. He proposed that the riparian owners should turn their attention to 'castoriculture' as a source of revenue, and thus enrich themselves while preserving the stock. Fortunately the old prize of 15 francs a head for all beavers killed, which was promoted in 1855, was suppressed in 1891 on the urgent representations of Mr Valéry Mayet. Since 1890, Mr Mingaud has kept a

record of all deaths which came to his knowledge, and he finds it average 8 to 10 a year. As he points out, Washington has established a colony of beavers in the National Park, and it is a complete success. The animals are kept to a woody valley through which runs a small water-course, and they there construct their dams and tunnels, quite familiarised to the occasioned presence of man, who can watch their daily life and works unheeded. He hopes that France will similarly protect the few last beavers remaining in the Camargue delta, and we cordially echo his sentiments.

A. T. Masterman 'on the Diplochorda'

Some preliminary notes by Mr Masterman in the *Proceedings of the Royal Society of Edinburgh*, for 1896, and also in the *Zoologischer Anzeiger*, paved the way for a more detailed paper of great interest in the *Quarterly Journal for Microscopical Science* (vol. xl., 1898).

The paper is divided into two parts—(1) on the structure of *Actinotrocha*, and (2) on that of *Cephalodiscus*. As the result of an exhaustive study of *Actinotrocha* (the curious larval form of *Phoronis*) by means of sections, Mr Masterman comes to the conclusion that the close similarity in structure to the three members of the group now commonly called Hemichorda points to a genetic connection, but that *Phoronis* and *Cephalodiscus* should be considered as constituting a distinct sub-division of the Chordata, for which the name of Diplochorda is proposed, owing to the possession by its members of paired lateral notochords. *Balanoglossus* is supposed to represent a later phylogenetic stage, in which these lateral notochords have fused in the median line. As there are certain animals which have always been objects of disagreement on account of their generalized types and the doubtful nature of their genetic relationships, so, too, there have always been certain organs or structures around which controversy has continually raged; the notochord is one of these, as students of the literature of *Balanoglossus* are well aware. If Mr Masterman's views are correct, and the paired structures he describes both in *Actinotrocha* and in *Cephalodiscus* are really of notochordal value, he has established a fact of the greatest scientific interest and phylogenetic importance, and we cannot but think he has made out a very good case for the homology. The figures on plates 25 and 26 are, we think, especially instructive, but they are nevertheless not universally regarded as convincing. Mr S. F. Harmer, whose views on the notochord of *Cephalodiscus* have been expressed in the *Zoologischer Anzeiger* (vol. xx., p. 342), while fully agreeing with Mr Masterman as to the relation of *Phoronis* with *Balanoglossus*, does not admit the homologies with *Cephalodiscus*, on which the main argument depends. Mr Masterman claims to show that the structure described by Mr Harmer as the notochord in *Cephalodiscus* is really the subneural gland, and

that lateral notochords, similar to those in *Actinotrocha*, are found in addition. The subneural gland, according to our author, is the same structure as that in Ascidians, and probably corresponds to the hypophysis in Vertebrata and the proboscis-vesicle ('Herzblase' of Spengel) in *Balanoglossus*. Mr Harmer puts the contrary view very fairly and clearly, but he is evidently at a loss to explain the alleged lateral notochords, if they are not what our author believes them to be. We hope that Mr Masterman will be able, in the absence of ontogenetic proof of origin (for *Cephalodiscus*), to bring more exact histological evidence to bear upon this vexed question, and to prove his point more completely.

Meanwhile, he has already done much in convincing us of the relationship between *Phoronis* and *Balanoglossus*, by comparison with *Actinotrocha*, and we are encouraged to hope for further light upon those matters which remain doubtful.

Change of Address

Once again *Natural Science* changes its address, and this time, not the address only, but the editorial staff. The reasons for this were given at length in our October number. They need not be repeated here; but we cannot omit to thank the numerous friends who have, since then, extended to us sympathy, both publicly and privately. We trust that all our subscribers, readers, and contributors will continue to support *Natural Science*, which, we have every reason to believe, will continue to deserve their support. There will be no change in the policy of the Review, no break in continuity, and no lowering of the standard hitherto set before it. But those who wish well to the future of this journal, should remember that it lies with them to see that it has a future. Editors cannot edit unless there are contributions of articles, notes, and news; publishers cannot publish if every reader reads the copy of a friend or of a library. Send at once your contributions and your subscriptions for Volume XIV. to the new editorial and publishing offices. The address is—Mr Young J. Pentland, 11 Teviot Place, Edinburgh.

And now the type-writer ceases for a moment to click, but the hands remain on the key-board. The days may have been anxious and the nights weary, but the work has brought us many interests and many friends, and it is hard to withdraw from it. Yet the last word must be written, and we write it with its oldest and fullest meaning—"Good-bye."

I

Mr Herbert Spencer's Biology [1]

WHAT strikes one most forcibly on reading the "Principles of Biology" in this new and enlarged edition is the extraordinary range and grasp of its author, the piercing keenness of his eye for essentials, his fertility in invention and the bold sweep of his logical method. In these days of increasingly straitened specialism it is well that we should feel the influence of a thinker whose powers of generalization have seldom been equalled and perhaps never surpassed. In no narrow or carping spirit should we approach the work of one who has done so much for the cause of evolution. We may set our queries against this or that matter of detail, or may enter a protest against the acceptance, without more conclusive proof, of certain broader principles; but we should not dwell on minor defects nor allow more grave differences of opinion to blind us to the gift which Mr Herbert Spencer placed in our hands thirty-four years ago, and has borrowed for awhile that he may return it to us with added weight.

The phrase 'added weight' is perhaps ambiguous. The volume of the work is materially increased, and new sections of much interest have been added. But though the intellectual weight has also been augmented, it is an open question whether it would not have been wiser to leave intact a treatise of such unique historical importance and value, relegating corrections and additions to notes and appendices. With all the labour and care Mr Spencer has expended on it during the last two years, it cannot be said that, having due reference to the contemporary state of knowledge in each case, the revised volume of 1898 holds the same position as the original work of 1864.

The scheme and method of the Principles of Biology and the manner in which Mr Spencer develops his subject are presumably so familiar as to render anything more than the briefest summary unnecessary. Starting in Part I. with the Data of Biology, the composition and properties of organic matter, the actions of external forces on it, its reactions to these forces, and its characteristic modes of metabolism, are severally discussed, and the conception of Life,

[1] "The Principles of Biology," by Herbert Spencer, vol. i. Revised and Enlarged Edition. 8vo, xii+706 pp. London: Williams & Norgate. 1898. Price 18s.

as involving the continuous adjustment of internal relations to external relations, is developed. To this part a new chapter on the Dynamic Element in Life is added.

After an indication of the scope of biology, the inductions of the science are considered in Part II. Generalizations as to growth, development, adaptation, genesis, heredity, and variation, are formulated and illustrated; the classification and distribution of organisms are considered, and the foundations are thus laid for the erection of an aetiological superstructure. The special-creation hypothesis is contrasted with that of evolution; the arguments for the latter are marshalled; and the causes of evolution discussed. Internal and external factors are distinguished; and the phenomena are explained as due to the joint action of (1) direct equilibration through the inheritance of acquired modifications, and (2) indirect equilibration through the survival of the fittest in the process termed by Darwin 'natural selection.' A concluding chapter on Recent Criticisms and Hypotheses, in which any inherent tendency to evolution along predetermined lines is rejected, brings the volume to a conclusion, save for Appendices, amongst which are the *Contemporary Review* articles on the Inadequacy of Natural Selection.

In the additional matter of the present edition Mr Herbert Spencer would probably lay most stress on the chapter which deals with the Dynamic Element in Life, and on the arguments in favour of direct equilibration through the inheritance of acquired modifications. While other additions, such as those on protoplasm, on metabolism, on nuclear changes, on embryological development, and on classification, serve mainly (for there are original suggestions) to bring the work into line with modern biological conclusions, the supplementary discussion of the Dynamic Element in Life is in touch with the author's distinctive philosophical tenets, and the arguments for direct equilibration are adduced in support of biological conclusions which Mr Spencer regards as of extreme importance. It seems desirable therefore to direct attention specially to these points.

After leading up to a conception of Life as the definite combination of heterogeneous changes, both simultaneous and successive, in correspondence with external co-existences and sequences, and after urging that the degree of life varies as the degree of correspondence, Mr Spencer briefly indicates certain vital processes which remain outside the conception as thus formulated, and contends that all cases "exhibit that principle of activity which constitutes the essential element in the conception of life." This he terms the dynamic element in life; and he asks whether it is inherent in organic matter or is something superadded. The notion of a super-

added vital principle is rejected. To the questions: Is there one kind of vital principle for all kinds of organisms, or is there a separate form for each? How are we to conceive the genesis of a superadded vital principle? Under what form does it exist in the dessicated rotifer?—to these questions the answers show that the alleged existence neither has been nor can be conceived. In attempting, on the other hand, to realize the dynamic element as "inherent in the substances of the organisms displaying it, we meet with difficulties different in kind but scarcely less in degree. The processes which go on in living things are incomprehensible as results of any physical actions known to us." "What then," he asks, "are we to say—what are we to think? Simply that in this direction, as in all other directions, our explanations finally bring us face to face with the inexplicable. The ultimate reality behind this manifestation, as behind all other manifestations, transcends conception."

One must not forget, in reading the chapter on the Dynamic Element in Life, that it forms part of a work which is itself only a part of a System of Philosophy. In Biology as a science it is questionable whether reference to the Ultimate Reality, and to noumenal as contrasted with phenomenal causation, is advisable. Phenomenal causation, as an explanation of natural occurrences, involves the reference of an event to a group of antecedent conditions of which it is the outcome. Noumenal causation, as an explanation of the totality of natural phenomena, involves their reference to an underlying *raison d'être*. The one deals with a chain of antecedents and sequents the ends of which, and its manner of support, are beyond the range of our mental vision as men of science. Of the other we can at best know or assume that it is. The phenomenal universe presents us with, or rather is, a series of data. Science explains their connections, and leaves to philosophy a discussion of their noumenal origin. But since Mr Spencer here treats Biology as part of a System of Philosophy the ascription of phenomena to their noumenal origin is not out of place, though one would have thought that a reference to "First Principles" should have sufficed.

And one cannot but think that Mr Spencer's treatment of the subject may render him liable to some misconception. On the first page of the "Principles of Biology" there stands now, as there stood in 1863, the assertion that "the properties of substances though destroyed to sense by combination, are not destroyed in reality. It follows from the persistence of force, that the properties of a compound are resultants of the properties of its components—resultants in which the properties of the components are severally in full action though mutually obscured." Further on we are told (in sections added to the present edition) that living matter "originated, as we must assume, during a long stage of progressive cooling," in which

occurred "the formation of molecules more and more heterogeneous." The inference which may fairly be drawn is, that the properties of living substance are the resultants of the properties of its components. But at first sight this does not seem to square well with the assertion that "the processes which go on in living things are incomprehensible as the results of any physical actions known to us." One can picture how certain folk will gloat and "chortle in their joy" over this confession, for such it will almost inevitably be regarded. But it is not likely that Mr Spencer is here, in so vital a matter, false to the evolution he has done so much to elucidate. The two seemingly contradictory statements are not really contradictory; they are made in different connections; the one in reference to phenomenal causation, the other to noumenal causation—to an underlying "principle of activity."

The simple statement of fact is that the phenomena of life are data *sui generis*, and must as such be accepted by science. Just as when oxygen and hydrogen combine to form water, new data for science emerge, so, when protoplasm was evolved, new data emerged which it is the business of science to study. In both cases we believe that the results are due to the operation of natural laws, that is to say, can, with adequate knowledge, be described in terms of antecedence and sequence. But in both cases the results, which we endeavour thus to formulate, are the outcome of principles of activity, the mode of operation of which is inexplicable. We formulate the laws of evolution in terms of antecedence and sequence; we also refer these laws to an underlying cause the noumenal mode of action of which is inexplicable. This, if I interpret him rightly, is Mr Spencer's meaning.

The use of the phrase 'dynamic element in life' is also open to misconstruction. A dynamic element, as understood by science, is a link in the phenomenal chain, and is expressed in terms of the inter-relation of the parts of a material system, and of actually observed attractions and repulsions. But it is customary for physicists to introduce at the outset of their discourse definitions of force as the cause or *raison d'être* of motion. These are now commonly regarded as a pious tribute to the noumenal, like grace before meat, and independent of physics in its strictly scientific aspect. But since such a definition of force not infrequently stands in the fore front of a treatise on dynamics, it may be regarded, noumenal though it be, as a dynamical postulate. It is in this noumenal sense that the term 'dynamic' is used in Mr Spencer's phrase. It is, we are told, a "principle of activity." And unless the reader is careful to note the distinction, Mr Spencer's position will be open to misconstruction. Vitalism is a philosophical conception; and the controversy which is suggested by the term is largely due to the

failure of the controversialists to distinguish carefully between noumenal and phenomenal causation.

The question may now be asked whether Mr Spencer is well-advised in attempting to comprise under one definition of life (1) certain fundamental attributes of living matter and (2) sundry complex results of evolution. Is not the correspondence between life and its circumstances rather the result of the interaction of vital factors, under the influence of the environment, than a primary characteristic of the vital factor itself? And if it be true that the degree of life varies as the degree of correspondence (which is open to question, since it is rather struggle than attainment which calls forth most strenuous vital energy) it is surely aggregate results, rather than fundamental properties, that we have in mind. If a unit must be selected, the cell, not the metazoon or analogous plant-complex, seems to be the more appropriate. But it is perhaps more profitable to fix attention, not on the variable unit, but on the common substance—protoplasm; to say that protoplasm has certain specified metabolic or other properties, that the processes carried on in virtue of these properties are what we comprise under the term life, and that evolution is the outcome under certain given conditions.

If Mr Spencer reply that the outcome in evolution is itself a fundamental attribute of life, it is difficult to see, in view of his position with regard to the dynamic element, on what *a priori* ground he criticises the hypothesis of determinate evolution—the orthogenesis of Eimer. To argue that the evidence of such determinate evolution is insufficient is a perfectly legitimate scientific position. But Mr Spencer goes further. He says: "The assertion that evolution takes definitely-directed lines is accompanied by no indication of the reasons why particular lines are followed rather than others. In short, we are simply taken a step back, and for further interpretation referred to a cause said to be adequate but the operations of which we are to imagine as best we may." If, however, we are to believe in a dynamic element in life, itself inexplicable, and if the outcome in evolution is in itself a fundamental attribute of life, it is difficult to see why the inexplicable determinate tendency should not be part and parcel of the inexplicable operation of the dynamic element. But if we regard evolution as the result of the complex interaction of vital factors, then we may fairly demand an explanation of the manner in which such interaction can give rise to orthogenesis.

It would be unprofitable (even if space permitted) to discuss at any length the pros and cons of what Mr Spencer terms direct equilibration. He is deeply committed to the inheritance of acquired modifications, and on this hinges much of his interpretation,

not only of biological, but also of psychological and sociological phenomena. But though this must to some degree influence his judgment, he is too honest and independent an enquirer into truth to champion a cause on these grounds alone. The pendulum of biological opinion tends to swing towards a negative position in this matter; and it is a distinct gain that the arguments in favour of the doctrine in question should be presented with all the logical force and power of exposition which Mr Spencer has at his command. And if there are many who still remain unconvinced, this is not for want of fresh arguments but because they feel the necessity for more facts.

Of the three lines of evidence on which Mr Spencer in large degree relies, the first is co-adaptation of co-operative parts. But until we know more accurately than we do at present what amount of co-adjustment is effected in each case by individually-acquired modifications, we lack important data for discussing the problem. Probably its range is very considerable. The horse runs and leaps with the added weight of his rider; and the work of all domestic animals of draught and labour shows that their organization will stand a strain far in excess of the normal. Granting that individual coadjustment will in each generation do much (just how much remains to be proved) it would seem that in the case of evolving antlers the added weight will for long be within the limits of such individual coadjustment. But it has been urged that the modification of a structure may foster, though it may not cause, congenital variations of like kind. For whereas other variations, since they are out of harmony with the circumstances of life, will constantly be eliminated in the struggle for existence, these are allowed free play. In other words, congenital variations coincident in direction with acquired modifications will be favoured. And there is no necessity for them to be accurately simultaneous; individual co-adjustment will make good the deficiencies of congenital co-adaptation. In view of such considerations the argument from coadaptation has little weight till we have fuller knowledge of the range of coadjustment.

The second line of argument adduced by Mr Spencer is the possession of unlike powers of discrimination by different parts of the human skin. But here again we need more facts. It is assumed that this discrimination is largely congenital. But we do not know what proportion of it is individually acquired. We do know that a comparatively short period of education in little-used areas largely increases, is said to double, the power of discrimination. And some psychologists contend that—as the term discrimination implies—what we are really dealing with is the special application of the power of central perception, not an increased delicacy of

peripheral nerve-endings. There is no spot on the skin that is not sensitive to the touch of a pencil-point. And we do not yet know the limits within which education and practice may refine the application of central powers of discrimination within little-used areas. The facts which Mr Spencer adduces may be in large degree due to individual experience; discrimination being continually exercised in the tongue and finger-tips, but seldom on the back or breast. We need a broader basis of assured fact.

It may here be parenthetically noted that Mr Spencer's contention that the nervous system is the result of direct equilibration is difficult to square with the embryological discovery that the axis-cylinders of the afferent spinal nerves take their origin in the nerve-crest (which differentiates into the ganglia on the dorsal roots) and grow outwards to their distribution in the skin or elsewhere.

A third line of evidence on which Mr Spencer relies is that supplied by vestigial organs, which, he contends, must be due to dwindling through disuse. But the explanation is beset with difficulties. Is there any evidence that a structure really dwindles through disuse in the course of individual life? Let us be sure of this before we accept the argument that vestigial organs afford evidence that this supposed dwindling is inherited. The assertion may be hazarded that, in the individual life, what the evidence shows is that, without due use, an organ does not reach its full functional or structural development. If this be so, the question follows: How is the mere absence of full development in the individual converted through heredity into a positive dwindling of the organ in question? In our present state of ignorance we can only adopt the form used by Mr Spencer and say: No reply.

It will be understood that the foregoing considerations are urged, not in support of one hypothesis or in opposition to another, but in advocacy of suspended judgment, of calm and impartial weighing of evidence, and, above all, of further observation and experiment. While the forces of battle are arrayed under the banners All-Sufficiency of Natural Selection and Inheritance of Acquired Modifications, there are non-militant biological agriculturalists who till the fields of observation and assert that they in truth provide the sinews of war. It is to them that we must look for conclusive evidence one way or the other.

We cannot part with Mr Spencer (only for a time it is hoped) without again expressing sincere admiration for his genius and gratitude for his self-sacrificing labours.

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II

Artificial Formation of a Rudimentary Nervous System [1]

(*d*) **Inhibition.**—Cl. Bernard's theory of nervous interference, can be corroborated by producing two excitations of an almost equal intensity at the ends of a big thread of mercury and by putting two halves of a tube of caoutchouc in the midst of the thread's surface. This tube performs the part of a heart, because its halves alternately approach and part at the passage of the waves. (Fig. 8.) Rest is doubtless the issue of the wave interference. (Action of the internal branch of the spinal.)

(*e*) **Formation of the dilated parts of the nervous system by means of wave interference.** — The continual excitations applied to both ends of the thread of mercury, originate the formation of a central dilatation. (Fig. 8′.) It, then, seems probable that the ganglions, plexus and dilated parts of the embryonic system, whose consistence appears to be even softer than that of adults, are owing to the continuous shocks of the vibrating waves that cause the unequal distribution of nutritive materials or the movement and concentration of the already constituted parts. Now, if the consistence of the neuroplasma increases, one may be sure that the construction of the definitive dilated parts has been finally attained.

This is, in fact, an extremely important cause of differentiation and consequently of progress. In higher animals the most continuous and intense sensations terminate in an excessive division of the nervous elements confined to the neuroglia. I have discovered that the consistence of the latter presents an exceptional importance. For instance, if you wish to obtain a great number of multipolar cells anastomosed almost in the same manner as those of the gray substance, you have but to place on some lard any viscous liquid (coloured albumen, saliva).[2] Even the action of a terminal resistance on a big thread of mercury (augmentation of consistence, inclination, etc.) suffices to obtain the claviform or cerebriform shape.

(*f*) **On the action of moderating nerves.** — These are

[1] Continued from p. 339. The figures referred to are on p. 334.
[2] Models of multipolar cells. *Natural Science*, August 1898.

probably traversed by some wave-currents endowed with a celerity that gives them, in some measure, the means to oppose themselves to the currents coming from the ganglia of the sympathetic system as well as to those issuing from the viscera. Some disordered movements of the heart, intestines, etc., are naturally the result of the suppression of the action of the brain and medulla. This theory ought to be applied in cases of vascular inhibitions.

(*g*) **On the part performed by the medulla.**—" Les résultats contradictoires de plusieurs physiologistes au sujet de la fonction des cordons antérieurs et latéraux de la moelle, tenaient aux modes divers d'excitation mis en usage. Vulpian a constaté qu'il faut une excitation très énergique pour déterminer les contractions dans les muscles recevant leur innervation des parties situées au dessous du faisceau excité : que les attouchements, les piqûres, les grattages superficiels ne produisent aucun résultat, mais qu'on met en jeu l'excitabilité de ces faisceaux en les pressant entre les mors d'une pince."[1] That is, until a powerful wave is produced. It is just the same with large masses of mercury, the vibrations of which cannot be brought about by rubbing them with a soft feather, and much less when the latter is protected by a cover.

" La substance grise de la moelle ne conduit point les impressions sensitives par des voies anatomiquement préétablies, mais pour ainsi dire d'une manière indifférente. Les sections transversales peuvent diviser la moelle epinière dans une grande partie de leur épaisseur, et dans un sens quelconque, sans interrompre la transmission des impressions sensitives, à la condition qu'une petite partie de la substance grise (une sorte de pont) ait été respecteé par l'incision. L'animal conserve la possibilité de reconnaître le point du corps irrité. Vulpian parlait d'une sorte d'empreinte originelle des sensations. . . ." The question is quite a simple one. The differences are only in the intensity of the vibration occasioned by the variability of distance, degree of excitation, point on which it worked, protecting envelopes, etc., and as a matter of course the excitations are conveyed almost in the same manner and each of them reaches certain points of the sensorium, however small the bridge of the gray substance may be.

Make a sort of a medulla with mercury and insert some conductors on its surface. (Fig. 10.) Some multipolar cells and conductory threads are then to be disposed in its upper part, as in Luy's methods. A stronger excitation will then rise higher than a weaker one and the elements placed at different heights will be diversely effected. Each of these reacts on the motor threads, but only in case it be sufficiently excited. (Fig. 10.) In this manner the perception of simultaneous sensations and multiple reactions

[1] Küss and Duval, l.c., p. 67.

are elucidated. For example: if you have a burning on your hand and another on your foot, they will be directly followed by two corresponding movements of retraction.

(*h*) **Reflexes or reflected circulation** (see fig. 11).—My theory alone can explain the law about the symmetry of reflexes (fig. 10), as well as those concerning intensity, irradiation and generalisation: this means that as the mechanical excitation grows stronger the vibration propagates itself more or less and produces a more and more general reaction. The same explanation can probably be applied to the phenomena of eccentricity. Any associated sensations, for instance the tickling, cough and nausea provoked in the pharynx by the pressure of a strange body in the ear, are to be explained by an excitation so strong as to radiate to the immediate centres of reflection.

Association of ideas consists perhaps but in the fact of a successive vibration; whenever some elements vibrate, such as stand near are set in motion by them.

"All our voluntary motions are in general associated, since we cannot move a single muscle separately but must needs move a group of them." And why so? Because the vibrations of the anatomic elements tend inevitably to radiate, notwithstanding inertia. This is observed in capillaries of mercury when examined under the microscope.

(*i*) **Influence exercised by the mass.**—Milne-Edwards says that the stronger an animal is the more mass its nervous system holds. This is made manifest in fig. 12.

(*j*) **Persistence of impressions.**—It is but natural that an excitation endowed with some intensity should produce a vibration that only dies away gradually. (Fig. 15.) Several optical delusions are probably owing to this cause.

(*k*) **Theory of sleep.**—The multipolar cells of the centres are known to have amœboid motions and to articulate and disarticulate themselves (fig. 19) by the discharge of CO^2 (?). This is exhaled in less proportion during sleep.

(*l*) **Model applicable to a general conception of a nervous system.**—I constructed a model in mercury, of the circulation of sensations and concomitant phenomena, modifying that (p. 48) presented by Luys in his great work on the brain. The most curious part of it all is that the vibrations provoked in the conductors are only reflected with sufficient intensity on the cells standing nearer the sensorium, and that they pass from thence to the big interior cells. There seems therefore to be no complicated mechanism whatever, everything appearing to result from a mere question of strength and distance. (Fig. 20.)

(*m*) **Evolution of the nervous system.**—The following experi-

ments need to be performed in a dark room, or at night, by illuminating the liquid with the somewhat oblique rays of a candle so as to obtain a marked shadow. Fill a floating dish with water and pour on its surface a small quantity of petroleum. (Fig. 21.) This forms large drops, on the edges of which appear certain pseudopodia with terminal spheres: these advance inwards originating therein several curious phenomena which are of no consequence at present.

The drops of petroleum playing the part of the neuroplasma or that of the nervous elements in evolution, project a distinct shadow on the white ground of the dish. Water is the actor intrusted with the all-important part of the neuroglia.

Some other liquids may likewise serve the purpose, since there is nothing indispensable, except the question of their densities and the indissolubility of the one in the other.

In fig. 21 there are five large masses of neuroplasm scarcely differing from each other (young foetus). But when any waves are produced in a single direction (acoustic impressions for instance) *A* and *B* are divided (fig. 22) and six different elements result from this mechanical division. Then, whenever the vibration continues with increasing intensity, one of the drops lengthens, adhering by one of its sides to the neuroglia or water, and finally constituting a kind of myelocyte, undergoes tension in several directions and sends the element *c* forth. We now have eight drops instead of the five primordial ones and a very small one, a little like the nervous embryonic cells (fig. 23). Still the experimenter goes on his task of differentiation and provokes many waves in two directions, obtaining thirty-seven more or less deformed drops (fig. 24). To close the experiment a new undulatory motion is provoked with a thick pin in a single direction and some currents of bipolar, multipolar, apolar, and articulated cells are formed, some resembling neuromata, while others are elliptical and have nuclei in them. (Fig. 24.) Though these figures are transitory I believe that permanent ones could be obtained by employing a melted grease which would preserve the shapes acquired by vibration as it cooled. The vibration can be provoked either in the water or in the floating drops of petroleum themselves.

Conclusions.—The origin of individuals and the construction of the organism by internal conditions is an exceedingly probable principle; even the origin and functions of the nervous system may generally be explained by vibrations, by waves running through certain conductors of neuroplasma and modifying it mechanically with regard to its shape, division, and connections. Every cause influencing general nutrition will modify the physical and chemical properties of the neuroplasma and of the neuroglia in whose bosom it slowly performs its boundless evolution.

Haemorrhages, commotions and circulatory disturbances; changes in the density and chemical composition of the blood, in the fecundating principle of the nerve which excites and awakes it just as the nitric or chromic acid set the mercury trembling; bile, that is a great excito-motor; inanition, inertia and fatigue; all that alters the compass of the vaso-motory systems or the nutrition of the neuroglia or of the neuroplasma may conduct to idiotism or frenzy.

There is a rise and fall also, an immense waste of nervous complications resulting from a small, simple, mechanical event. Now, when any shapeless, primordial masses of neuroplasma are for ever vibrating and dividing themselves, be it by work, by exalted excitations of the sensorial impressions, by innumerable congestions, or by hunger, love, strife, meditation, millions of small, light, plastic, movable cells issue, that are incessantly articulating and dislocating themselves. Intellectual perfection could otherwise not be conceived. Broca observed that when the faculty of speech was lost by alterations of the frontal centre, it could be gradually recovered by a development of the opposite hemisphere. In short, the evolution of which the encephalon of man and animals is susceptible by means of education, demonstrates that the systems and divisions of nervous elements are not invariable, but perfectible and variable, and that the neuroplasm keeps for a longer or lesser time the shapes acquired, according to the degree of its density and vigour, several circumstances, such as age, vivacity of first impressions or vibrations, its repetition, and so forth, being at all times prevailing.

Physiologists evince an inveterate electro-mania in their exertions to explain everything by the action of electricity, taking the negative variation together with the fact of the existence of electric fishes as a principle. Well, notwithstanding the minute investigations of Du Bois Reymond and others, they have never been able to explain anything by such means, not even the influence of compression upon the nerve, or the muscle's vibration. Besides, Marey and Moreau have demonstrated that the electrical apparatus of the *Torpedo* works as a muscle and has almost muscular jerks; second, that the nerves with which it communicates do not carry the electricity to this queer machine, having only the faculty to set it to work. Moreau proves that when the prisms are treated with various reagents the discharges are not modified, but that they come to a stop when the former coagulate the albuminoid bodies, that is, soon after the physical conditions of the phenomena are modified. I suspect that nervous vibration has a mechanical action there, and that electricity unfolds itself on account of some rubbing or vibrations in the separating partitions of the 100,000 or 200,000 close-packed prisms. Becquerel says it is enough to press a disc of cork on

an orange and to separate it rudely afterwards to load it with a considerable quantity of positive electricity.

. . . . . .

I conceive the human organism, therefore, as a machine containing some five or six litres of blood employed in appropriating to itself the nutritious principles of food, absorbing oxygen, and carrying it to the nerve to make it vibrate by discharges of carbon dioxide.

Yet, this machine being magnificent, it were suitable to imitate it by means of some very sensitive springs (muscles) moved by the vibrations of a great number of semi-liquid conductors (nerves) in communication with proportionated deposits or centres.

The inheritance of nervous affections or that of the attributes of genius may, by consequence, be easily explained. Whenever the progenitors have a strong, healthy, and active protoplasma, the nutrition of the embryonic neuroplasma of the descendants is perfect. All will, in fact, be correlatively nourished, and the germs united by molecular attraction will form a strong gastrula, a superb neurochord, and later still the three fundamental embryonic brains.

Féré's observation regarding the transmission of such nervous affections, epilepsy and the like, as manifest themselves so soon as the general conditions of nutrition improve, can be elucidated thus, and likewise the singular immunities of wise men as indicated by foreign physiologists, and Chalumeau's famous law.

A. L. HERRERA.

MEXICO, *May* 1, 1898.

[It should be explained that Professor Herrera wrote this article in a language with which he is unfamiliar, and that he has had no opportunity of correcting the proofs.—ED. *Nat. Sci.*]

595.78

III

The Neuration of Rhopalocera

GIVEN the theory of evolution, structural characters of living insects, whether embryological or imaginal, afford means of classification according to the persistence among them of various degrees of specialization from primaeval forms of structure, *i.e.* the links of continuity of specialization in any direction. The neuration of the wings is a structural character common to all Lepidoptera —a few apterous females excepted. Hepialides and Micropterygides, perhaps the most ancient groups of Lepidoptera now existing, have more wing nervures than any other group and associate the Lepidoptera with Trichoptera; series of gradations in modification of neuration in different directions more or less connected (less connected, perhaps, among ancient than recent groups), may be observed everywhere among existing Lepidoptera; thus reduction in the number of nervures, or alteration in the position of certain nervures, connect generalized (ancient) and specialized (recent) forms of neuration; as a matter of observation, I believe that, nervures once lost, or the position altered, neither has ever been regained—hence the different series of gradations.

Fore and hind wings corresponding, the neuration may be referred to as follows:—Costa = upper margin. (1) Subcostal nervure. (2) Radial system = nervure with branches. (3) Median system = nervure with branches (nervules). (4) Cubital system = nervure with branches (nervules). (5) Anal nervures = several simple (unbranched) separate nervures.

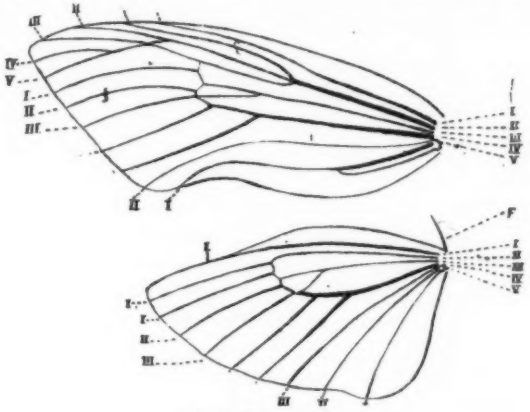
In this paper I propose mainly to refer to the neuration of the forewings, and possibly shall not enumerate all the nervures of the wings to save unnecessary details.

Cossids, so far as I know, cannot be considered in any way related to the Rhopalocera, except by connections too remote to trace; they retain a neuration relatively more ancient than that of any group of Rhopalocera.

It may be instructive to compare the Cossid form of neuration with the several more or less definite forms representing several groups of genera in the Rhopalocera, and I may here say that by the term group I mean an assemblage of species which may be associated upon a given pattern of wing neuration, or a distinctly connected modification of the same. In a recent instructive paper upon the "Classification of the Day Butterflies," published January and February 1898 in *Natural Science*, Dr Radcliffe Grote has given details of

the modification of neuration to be observed in the different groups, and I will briefly refer to some lines of specialization observed.

GROUP 1.—Generalized Hesperids (*Hesperia*) retain five-branched radius in forewings, and three median nervules in very nearly the ancestral position; but a modification of the transverse 'cell' nervure in the direction of the cubital system forms a three-branched cubitus in forewings (ancestral form is two-branched as in Cossids). Among



Australian Cossid.

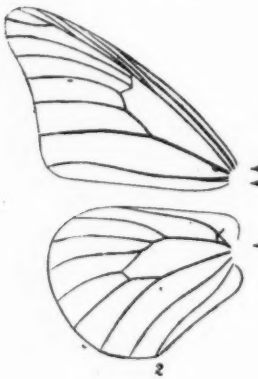
specialized Hesperids (*Pathesperia*) one median nervule by incorporation with the radial system forms a six-branched radius in forewings, the middle median nervule and the cell nervule in hindwings disappear, and there is a tendency in the middle median nervule in forewings to do likewise. (See Dr Grote's figures; *Nat. Sci.*, vol. xii., pls. i. and ii.).

The neuration of Lycaenids is similar to that of Pierids, but not identical; in each group specialization by reduction in the number of radial nervules in forewings is observed, but there is this difference, viz., whereas among Lycaenids reduction occurs while the median nervules retain their position and identity as such, the same specialization is observed among Pierids after the incorporation of one of the median nervules with the radial system. Whether it is possible to take into consideration the relative position of the median nervules in the forewings of Lycaenids and associate them with Hesperids therefor, my personal observations are not sufficiently extensive to justify an opinion. Certainly the position of the median nervules is extremely alike in each group, and Dr Grote associates them, having observed also the tendency among Lycaenids to lose the middle median nervules in the same manner as do Hesperids. This, so far as

Chrysophanus phlaeas (Lycaenid).

I know, has not been observed in any other group. The neuration of Lycaenids is specialized compared with that of the generalized Hesperids, and sufficiently distinct from that of Pierids and allied groups to warrant the conclusion that the latter are as far removed from Lycaenids as they (Pierids, &c.) are from Hesperids. Lycaenids and Pierids have attained a similar specialized form of neuration independently and by different routes. Grote writes, "Lycaeni-Hesperidae meet upon a distinctive wing pattern, the Lycaenidae differ in the main by the reduction of the radial branches."

Group 2 (A).—Generalized Nymphalids, (Danainae, &c.) retain five-branched radius, have distinct three-branched cubitus in forewings, and retain the 'discoidal cell' transverse vein of each wing. *Anosia* has a structural 'blotch' (A) towards the base of the cubital system, and a rudimentary nervure (B) at the base of the anal nervure in forewings. The latter feature is observed in other groups, and occurs frequently among the Heterocera. *Anosia* has also a subcostal-radius connection in hindwings (C). Specialized Nymphalids show a gradual loss of the transverse 'cell' nervure of each wing, complete loss being attained (*Apatura*, *Junonia*, &c.) without incorporation of the median nervules with the radius system of the forewings—the two median nervules remain as such, attached to the radius by a modified remnant of the 'cell' nervure.



Anosia archippus
(Nymphalid).

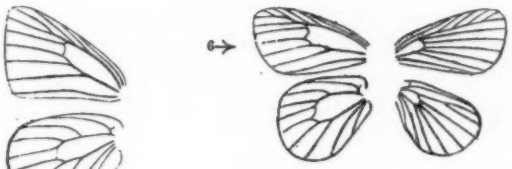
Enodia hyperanthus
(Satyrid).

Satyrids may be associated by descent with generalized Nymphalids; the radius system is five-branched, and the 'cell' nervures are retained throughout the group, the wing pattern is distinctly nymphalid. The features A, B, C of *Anosia* are not observed among Satyrids.

Junonia orithya
(Nymphalid).

Group 2 (B).—Generalized Pierids. *Leucophasia* is isolated among the Rhopalocera in respect of the wing pattern, and the neuration must be regarded as specialized, especially in regard to the arrangement of the radial branches (five) of the forewings. As a Pierid, however, having five-branched radial system, two median nervules, and three-branched cubitus in forewings, *Leucophasia* affords a connection between the generalized Nymphalid form of neuration, and

the next in sequence among the Pierids, viz. *Euchloe*, in which incorporation of one of the median nervules with the radial system of the forewings forms (as in Hesperids) a six-branched radius.



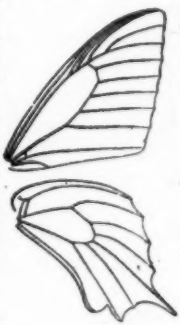
Pieris daplidice.

Euchloe cardamines (Pierids.)

Leucophasia sinapis.

Further specialization among Pierids is by reduction in the number of radial nervules in the forewings, and the movement of the remaining median nervule towards the radial system. The transverse 'cell' nervure is a permanent feature throughout the Pierid group.

GROUP 3.—Papilionids. The forewings have five-branched radial system, four-branched cubitus, only one median nervule remaining as such, a transverse cubitus and connection near the base of the wings, and two anal nervures. The hindwings have a subcostal-radius connection, a very persistent feature, which varies only so far as to form larger or smaller 'cells' in different genera. One anal nervure only is typical of Papilionid neuration. All other groups (*i.e.* Groups 1 and 2) have two anal nervures in the hindwings.



Papilio machaon (Papilionid).

From these notes it will be seen that however widely the specialized Rhopalocera may differ, the generalized forms of neuration have features in common—five-branched radius, two median nervules as such, three-branched cubitus in forewings (Groups 1 and 2). Dr Grote believes it is "uncertain" that the short anal nervure of Papilionid forewings, is homologous with the rudimentary anal nervure in the forewings of other groups, and conceives a separate origin for Papilionids. In such groups of Heterocera as have two anal nervures in the forewings, a rudimentary nervure is often present (see Cossids), always at the base of that anal nervure farthest from the cubital system. Comstock figures *Megalopyge crispata*, a 'low' moth which has an extension of the rudimentary nervure, a remnant of a third anal nervure in the forewings. Most Rhopalocera have lost one anal nervure (nearest the cubital system), a loss observed in many Heterocera; Papilionids retain two, one of which has become rudimentary in other groups of Rhopalocera (also in Cossids and many

Heterocera). Here also we may refer to the features A, B, C of *Anosia* (evidently overlooked by Dr Grote); as already stated, these are not to be observed among specialized Nymphalids. Now, the cubitus-anal connection, short anal nervure of the forewings, and subcostal-radius connection in the hindwings of Papilionids are generalized features, homologous with the features A, B, C of *Anosia*, modified and lost in other groups of Rhopalocera; it is impossible to regard them in any other light. In this respect then Papilionids approach the generalized Nymphalids.

Reaction of modification appears to me to have caused different lines of specialization of neuration in the Lepidoptera, consistent with adequate support of the wing membrane, and the development of wing area and shape; for example, it seems that modification of neuration has been rapid in the case of *Opostega crepusculella*, which is not highly specialized in wing area and shape as compared with primeval Trichoptera-Lepidoptera, but the neuration has been reduced to several simple (unbranched) nervures. The area and shape of Rhopaloceran wings are, however, more highly specialized, and the modification of neuration—particularly of the forewings—has been less rapid. If we draw the line separating primitive Rhopalocera from the mass of primaeval Lepidoptera at Hesperids, then we have the fact that no Rhopalocera possess the basal portion of the median nervures, and this loss we may assume to be the point of separation (granting, also, modification of hindwing neuration, which everywhere in the Lepidoptera has preceded that of the forewings). Next we have noted the formation of three-branched cubitus in the forewings, by the incorporation of one of the median nervules with the cubital system in Hesperids and subsequent specialization in the direction of Lycaenids. In another direction, apparently at some distance from Hesperids, we note that a three-branched cubitus is a fixed feature in the neuration of Nymphalids' forewings, that no movement has taken place in the position of the other median nervules, and very little subsequently among Nymphalids and Satyrids. But as the Pierids separated from the Nymphalid-Pierid stem, the median nervules moved towards, and one became incorporated with, the radial system. The cubitus 'blotch' and rudimentary anal nervure of the forewings and the subcostal-radius connection in the hindwings of *Anosia* point to the possession of the Papilionid cubitus-anal connection, two anal nervures in the forewings, and a subcostal-radius connection in the hindwings by those primaeval Rhopalocera from which the Nymphalid-Pierid groups originated; and another important fact is that at this point two anal nervures in the hindwings were retained (as in Hesperids). Next in sequence come the Papilionids, in which movement of one of the (Nymphalid) median nervules forms

a four-branched cubitus in the forewings, the generalized cubitus-anal connection, and the two anal nervures of the forewings being retained, likewise the subcostal-radius connection of the hindwings. One anal nervure only, however, is retained in the hindwings; that is to say, one of those present in Nymphalid-Pierids has been lost.

My conclusion from a study of the neuration is that the distinct separation of Papilionids in phylogeny from the other Rhopalocera cannot hold good, and in this I join issue with Dr Grote. Whether I have clearly demonstrated this in the above, I cannot say; but the facts are distinctly in evidence that, however widely the specialized forms differ, the generalized forms of neuration indicate a natural sequence in modification—the evolution of the Papilionid form from a primaeval form such as is illustrated by Cossids; and of this evolution the generalized Hesperids (1), Nymphalid-Pierid (2), and finally generalized Papilionids represent the surviving links in the continuity of specialization, a primary modification of neuration antecedent to and quite apart from the special modifications peculiar to the several groups.

I need only refer to Dr Chapman's paper, "Butterfly Pupae" (*Entom. Record and Journal of Variation*, vol. vi. pp. 106 and 125), to support my conclusions: "The lowest (*i.e.* most ancient) forms in all the families are really very close together."—"Pierid and Nymphalid started together, shortly afterwards separating." Nymphalid pupae are capable of lateral movement only, as are Pierids, but have lost the 'girth' which is characteristic of the Pierid and Papilionid method of pupation; Nymphalids, however, retain the 'double nosehorn' pupal structure of Papilionids (*i.e.* a generalized pupal character), Pierids having lost this particular pupal character. The 'double nosehorn' is surely corroborative of my conclusion that the features A, B, C of *Anosia* are homologous with the Papilionid cubitus-anal connection, short anal nervure in forewings, and subcostal-radius connection in hindwings, which establish affinity between the Nymphalid-Pierid and Papilionid groups.

If Papilionids be separated from Nymphalid-Pierids in the manner proposed by Grote, then the neuration of Papilionids must be regarded as isolated, highly specialized, and without surviving forms connecting it with primaeval neuration from which it has been derived. On the other hand, the "primary modification of neuration" among primitive Rhopalocera shows the formation of the three-branched cubitus in forewings, with two anal nervures in hindwings; then follows the formation of the four-branched cubitus, with loss of one anal nervure in hindwings. This is more in accord, I believe, with the evidence of development, and supplies the connecting links between the primaeval neuration and that of the highly specialized Papilionid.

A. QUAIL.

PALMERSTON NORTH, NEW ZEALAND.

IV

A Theory of Retrogression

IT is widely believed that the development of the individual is a recapitulation of the life-history of the race. In other words, it is believed that every individual begins life as a unicellular animal, the germ, and then, in a very rapid indistinct fashion, represents, in orderly succession, all its long line of ancestors, till in the end it represents its parent. This recapitulation is not more wonderful and mysterious than any other fact of biology. Imagine the primitive world, in which only unicellular organisms were present. Suppose that variations occurred amongst these, just as we know they occur higher in the scale. Then we may well believe that such variations as the following occurred—that, when one cell divided into two, the resulting cells did not separate, as normally happened, but remained adherent. This variation, which, like other variations, would tend to be transmitted, and which, if fortunate, would tend to cause the ultimate survival of the organisms which possessed it, would be the first step in the evolution of the multicellular from the unicellular organism. The dual animal which resulted would reproduce by each of its cells dividing into two, so that there would be four single cells which would separate, so as again to form unicellular organisms. But each unicellular organism would, in general, inherit the peculiarities and repeat the life-histories of its grandparent cells by dividing into two adherent cells. A race of two-celled organisms would thus be established. We may fairly believe that in time a second variation, which also proved fortunate, occurred, whereby the four grand-daughter cells also remained adherent until reproduction; and afterwards other variations of the like nature, till an organism was at length evolved which consisted of a multitude of cells adherent for the common benefit. When this organism reproduced it would be by one or more of its cells separating and dividing into two adherent cells, these into four, and so on, till the parent organism was represented. Ontogeny would thus necessarily recapitulate phylogeny. This rule would still obtain when evolution proceeded farther, and cells had become differentiated and specialised for the performance of different functions. Every individual would still begin as a single cell, the germ, and then, step by step, would represent ancestor after ancestor till, at last, he represented the last of the race, the parent. The above view of heredity is necessary to my argument, and apparently is opposed to other and more modern

theories which at present seem to hold the field—for instance, Weismann's theory of Germinal Selection, or Mr Francis Galton's theory that so much of an individual is derived from this ancestor, so much from that, and so much more from a third. Every one of these latter theories ignores what seems to me the patent fact that the characters of all the ancestors are not commingled in the final result, the adult, but that during ontogeny each parent is represented in turn. It is true that watching the development of an individual, we cannot say that at such and such a point the great-grandparent ends and the grandparent begins, that at this other point the grandparent ends, and behold—the parent! The changes are too complex and subtle, too swift and fleeting; moreover, at every turn the variations from his ancestry of the individual under observation strike in and add to the apparent confusion.

It may be objected that the child during his development does not represent exactly, nor even closely, any of his remote ancestors; and this objection would appear fatal to the above theory of heredity. On the other hand any sufficient explanation of this vagueness of representation will go far to establish, not only this theory, but also that theory of retrogression which is the subject of this article, and which—if it be a true theory—is in a humble way the complement of the theory of evolution.

Offspring, as we know, vary from their parents, and if they vary, they must do so primarily in one of two ways. Either they must revert to the ancestral type, and resemble it more than the parent did, or else they must diverge from it still more than did the parent. The former variation we term 'atavistic,' the latter we may term 'evolutionary,' since it is on the lines of these latter variations that evolution proceeds. But of so-called atavistic variations, there are also two kinds; one of which is really atavistic or reversionary, whereas the other, though apparently atavistic, is actually evolutionary. True reversion occurs only when the individual varies so from his parent that, in his development, he does not recapitulate the whole of the life-history of his race, but stops short at a point reached by a more or less remote ancestor, whom in this way he resembles more than he does his parent. False atavism occurs when the individual, at an early stage of his existence, begins by recapitulating the whole of the life-history of his race up to his parent, but during a later stage retraces, or apparently retraces, some of the last steps made by himself in his development and by the race in its evolution, and thus, by a species of evolutionary variation, resembles a more or less remote ancestor more than he does the parent. Examples of this false kind of atavism are plentiful in nature.

The points here set forth are these. First, that development is a recapitulation of evolution, in other words, that every individual

repeats, though very rapidly and indistinctly, the life-history of his race, beginning with the unicellular organism and ending, in many cases, with the parent. Secondly, that an individual may so vary from his parent that he does not recapitulate the whole of the phylogeny, and that this constitutes true atavism, true reversion. Thirdly, that there is a false atavism, which is really evolution. This occurs when an individual after reaching the full development of his parent retraces some of the last steps of the ontogeny, and so resembles an ancestor more than he does his parent. More need not be said concerning the first proposition. As regards the third, it has been said above that examples of false atavism are frequent. From the nature of the case observation of it is difficult; for in every individual this retracement of the ontogeny, this false atavism, must be very slight—so slight as usually to be inappreciable. Therefore it is only by observing the retracement, not in an individual but in a line of individuals, that it becomes plainly noticeable. It is by taking advantage of such retracement that 'Reversed Selection,' as it has been termed, eliminates a structure which a change of environment has rendered not only useless, but worse than useless, more rapidly than would otherwise occur under the mere absence of selection. For example, Natural Selection has resulted in the evolution of eyes. In animals dwelling in absolute darkness, *e.g.* certain cave-dwellers, the eye has become not only useless, but worse than useless, since it is an extremely prominent and tender, and therefore vulnerable, part of the organism. In some such animals we observe that the eye is better developed in the embryo than in the adult. Clearly here the animal in its ontogeny retraces some of the steps it has already made. Clearly also, if ontogeny be a recapitulation of phylogeny, such retracement was made in the phylogeny as well. It follows that when a structure, useless both to the embryo and the adult, is better represented in the former than in the latter, it must have undergone retrogression through the action of reversed selection, and that during the phylogeny, after being useful, it became not only useless, but worse than useless.

The second proposition, that an individual may so vary from his parent as not to recapitulate the latter stages of the phylogeny, and that this constitutes true atavism, is the main proposition of the present thesis; but I have yet to prove that this atavism is the cause of true retrogression.

True atavism can seldom be observed in such of the higher animals and plants as have been evolved under Natural Selection, not because it does not occur, but simply because it is usually masked and slight. It is masked because such complex beings seldom or never retrogress in all their characters at once, and, therefore, such reversion as may occur in this or that particular is associated with

evolutionary variation in other particulars. It is slight because, since such species have evolved but slowly, reversion to a not very remote ancestor does not result in any appreciable change of type. Thus, under ordinary circumstances, if a man reverted in any particular to an ancestor of a thousand years ago, no one would recognise to what the change of type was due. Not only would the change be too slight, but the observer would need to have a knowledge of the ancestral form, and such knowledge is usually impossible. Sometimes, however, recognisable reversion does occur even among such beings. Thus a man may resemble the portrait of some far-away ancestor, or again the progeny of an ordinary pair of horses may exhibit the zebra-like stripes of a remote ancestor. It is not, however, among complex beings, slowly evolved in every particular, that we must seek our proofs. We must turn to plants and animals that have undergone swift evolution in some one particular, and this, so far as I know, occurs only under stringent Artificial Selection. For Natural Selection, having care for many characters, results in but slow evolution; but Artificial Selection, having care for only one or only a few characters, results in much swifter evolution. Supposing, then, we take any breed of domesticated animals or cultivated plants, and, after choosing the finest specimens, henceforward breed indiscriminately from these and their descendants; what then happens? It is notorious that under such circumstances cessation of selection is marked by a reversion towards the ancestral type, a reversion swift in proportion to the swiftness of the antecedent evolution. Thus, without continued stringent selection, the speed of race-horses cannot be maintained; they tend to lose their special characters, and revert to the ordinary horse. The same is true of all other prize breeds. Again, careful breeding from ordinary horses readily evolves a speedier race, for the offspring of ordinary horses in many instances surpass the parents. But, in proportion to the success of the breeder, further improvement grows continually more and more difficult, till at length evolution practically reaches a standstill. Improvement thereafter is very slow indeed. For this reason it is now very difficult to improve our breed of race-horses. The offspring of a pair of the finest animals are in the great majority of cases inferior to their parents, and, therefore, practically all that the most stringent selection is now able to achieve is to preserve, not to improve, the race. It is, therefore, plain that, owing to the increasing tendency towards reversion, rapid evolution quickly slows down, till, even in the presence of stringent selection, it practically ceases.

But perhaps the most striking proofs of the present theory are furnished by certain cultivated plants (for instance the apple), which are usually propagated by means of slips or suckers—that is, by detached portions of the individual. Practically speaking, the most

favourable individual of a species has been chosen and multiplied by means of slips, the rest of the species being eliminated; and in each new seminal generation the process has been repeated. Such plants, therefore, have been evolved by a tremendously severe process of selection, resulting in an evolution much more rapid than is possible among animals or annual plants. But now supposing we chose any one of these highly divergent varieties, and without using any selection, bred from seed alone, what again would happen? There is ample evidence leading us to believe that in the vast majority of instances the variety would swiftly (that is, in a very few generations) revert to something very like the wild stock from which it originally descended, but not to the wild stock precisely, for, no doubt, while the cultivated species was undergoing evolution in one direction, it was, under the changed conditions, undergoing retrogression in other particulars, and in these the reverted varieties would differ from the wild stock.

I need not dwell longer on the tendency such plants and animals have towards retrogression. The facts are notorious. But it seems to me that these facts are strongly adverse to all those recent theories of heredity to which I have alluded, and which suppose that each ancestor is not represented in turn during the ontogeny, but that the characters of all or many of the ancestors are commingled or are latent in the final result, the adult—Weismann's theory of germinal selection for instance, or Mr Galton's theory, which supposes that, on the average, one quarter of the total heritage of an individual is derived from the parent, one-eighth from the grandparent, one-sixteenth from the great grandparent, and so on. Were such theories true there could be no retrogression except through reversed selection, for the more evolved ancestors would for ever tend to make their influence felt. But plainly retrogression occurs in the mere absence of selection. Moreover, if it be true that the organic world has arisen through the preservation and accentuation of favourable variations, and if it also be true that ontogeny is a recapitulation of phylogeny, then it seems to me that it must be further true that there is necessarily a greater tendency towards retrogression than towards evolution. For all atavistic variations must tend towards retrogression; whereas all evolutionary variations need not constitute extensions of the previous evolution. They may result in divergencies in new directions; or may even constitute reversals of the previous evolution, as in those cases of which Reversed Selection takes advantage. Given sufficient time, in the absence of selection, retrogression must therefore necessarily ensue.

The rationale of retrogression, I take it, is as follows:—Suppose, as regards any character which has undergone evolution,

that A B C D represent a line of individuals; then if D reverts to B, that is if D varies from his parent C in such a way that in his ontogeny he represents the life-history of the race only up to the point reached by B, omitting the additional characteristic of C, it is evident from the point of view of heredity, that the series becomes A B D; or rather it becomes A B, since in effect, D is B. C then disappears completely and for ever from the series; and it follows that, if the characters of C ever reappear in E, or any subsequent member of the series, they must do so as a result of fresh evolution, not as a result of reversion. It is necessary to emphasize this point for on it my whole argument depends. If D, on the other hand, varies in such a manner from C that after representing C, that is after recapitulating the whole of the phylogeny he reverts back to B, then C does not disappear from the series. C will still be represented in the ontogeny, and, if his characteristics reappear in any individual at the end of the ontogeny, that is in the adult, it will be as a result, not of evolution, but of reversion. As I have already indicated, it is on such cases as the latter that Reversed Selection works. Thus, when during the phylogeny any character becomes useless and selection ceases, retrogression eliminates it with a speed which is proportionate to the speed of evolution. But, if it becomes worse than useless, then an additional factor steps in to hasten the elimination. Reversed Selection then takes advantage of such apparently atavistic, but really evolutionary variations as cause an individual, after he has represented his parent, to revert back again to a remoter ancestor. Moreover Reversed Selection not only preserves such individuals, but also eliminates all such individuals as have the worse than useless characters in a greater degree than their parent, and thus prevents them from influencing posterity.

It would be well to illustrate the foregoing with a concrete case. Suppose we plant seeds of those garden plants which I have instanced as having undergone very swift evolution. In a great number of cases the young plants revert towards the ancestral wild type. Now I have enquired everywhere, and I have never heard that the seeds of such a reverted plant, or of any of its descendants have ever reproduced the cultivated type. This means that the cultivated type has disappeared absolutely from the series. It will never again be represented in the ontogeny, and could reappear only as a consequence of fresh evolution, resulting from selection as stringent as that by which the cultivated type was originally evolved; if it did reappear without fresh evolution it would be because the reversion to the wild type had resulted not from true atavism, not from a lapsing of the last steps of the ontogeny, but from the false atavism on which Reversed Selection works. But,

since the retracement on which Reversed Selection works is apparently always small in amount, it never seems to occur in species that have been so rapidly evolved as these garden plants. Their reversion, therefore, seems to be invariably due to true atavism, there being apparently no room for Reversed Selection. Here, then, is a strong proof, convincing proof as it seems to me, that true atavism means a lapsing for good and all of the last steps made in the phylogeny.

Two things are evident from the foregoing. First, that there is on the average a greater tendency towards reversion than towards evolution, that is, there is a greater tendency to revert towards the ancestry than away from it, in other words, there is a greater tendency to let lapse in the ontogeny the last steps made in the phylogeny than to add other steps to them. Secondly, the strength of the tendency towards reversion is proportionate to the swiftness of the antecedent evolution, and, therefore, species which have been quickly evolved, tend to retrogress swiftly, whereas species, which have been slowly evolved, tend to retrogress slowly. For this reason it is that characters long established in the species are much more stable than more recent characters, for, in the former case, reversion, to be appreciable, must be to an extremely remote ancestor, whereas in the latter, reversion to a much less remote ancestor results in appreciable retrogression.

Suppose now a certain character in a line of individuals has undergone evolution. Denote by the symbols A B C D E F, the evolution of the character in successive individuals of the line, A being the rudimentary character as it appeared in the first of the line who had it, F the character when it reached its highest perfection. Suppose that cessation of selection occurs as regards this character. Then F tends to be lapsed, and, when it is lapsed, E reappears at the end of the ontogeny. But thereafter E also tends to be lapsed, and D to reappear, and so on, till, in the continued absence of selection, at length A reappears. But under the same law A tends likewise to disappear, and then the character vanishes utterly, and the race reverts to that ancestral condition when the character did not exist. In this manner I take it do useless parts disappear absolutely. Thus have disappeared, for instance, the limbs of the snake. Thus have disappeared the eyes of some cave-dwelling animals, and the many useless parts of parasites. Thus have vanished innumerable useless parts in every plant and animal.

We are now in a position to consider the part played by reversion in nature. Every complex individual, as we know, varies in a thousand ways, great and small from its parent; but only here and there is a variation useful. The useful variations, in proportion to their usefulness, are preserved and, in succeeding generations, are

accentuated by Natural Selection. The useless variations, the vast majority, are planed away by reversion. Most of them being minute, disappear in the next generation, but, even when they are comparatively great, a very few generations suffice to procure their disappearance. Even should a series of individuals happen to vary in such a manner that in each successive individual a useless character is more and more accentuated, yet, since the tendency towards atavism is greater than towards evolution, a time surely comes when, perhaps in a single generation, the whole of the evolutionary variations lapse and the character vanishes, never to reappear, except in the improbable event of fresh evolution of a like nature. Again it sometimes happens that a change of environment renders useless a structure which was formerly useful. Here also reversion steps in and procures its elimination. Such a structure—say the wing of a bird, the habits of which have ceased to be aërial—was evolved by the superimposition in a long line of individuals of favourable variation on favourable variation. These, when the character becomes useless, are lapsed in orderly succession, the most recent first, the more ancient later; till, at last, the structure reverts to that most ancient condition when it did not exist. In this manner it approximates continually to more and more ancient forms, but only approximates. It never reproduces its prototypes of the phylogeny exactly, for during the whole course of evolution, reversion was at work, planing away everything which was originally useless, or which became useless as the environment changed. A complex organ such as a wing is, therefore, a product not only of evolution but also of reversion. Evolution roughhews the organ, but reversion chisels its finer lines. What is true of a complex organ is true in a yet greater degree of every complex plant and animal. Such a being is a product not only of evolution, but also of reversion. In it many structures, useful during a remote period of the phylogeny, but useless later, have disappeared utterly by reversion to that yet more ancient condition when they had not come into existence. Others, in which reversion is yet incomplete, still persist, and are known to us as vestigial remains. It should, however, be noted that, when a vestigial structure is more developed earlier in the ontogeny than it is later, this indicates that its retrogression is due not only to reversion the result of true atavism, but to false reversion the result of Reversed Selection. Such a structure must have become not merely useless, but worse than useless during the phylogeny.

Every complex animal, therefore, in the successive stages of its development does not represent exactly successive stages in the evolution of its race. At each stage of the ontogeny are present useless structures, or useless parts of structures, which have retro-

gressed backwards towards a more ancient order of things; and at every stage of the ontogeny structures are absent, which were present in the phylogeny because they were then useful, but which since underwent complete retrogression, because they subsequently became useless. Here, then, we have the explanation of the fact that ontogeny is only a very vague recapitulation of phylogeny. Doubtless if a higher animal, a man for instance, lived during his ontogeny in a succession of environments similar to those in which his race was evolved, his ontogeny would much more exactly recapitulate the phylogeny than it actually does, for in that case structures, which had been useful during the phylogeny, would continue to be so during the ontogeny, and so would be preserved. But consider how vastly different is the environment, in which the embryo of man develops, from the environments in which his race evolved. The embryo develops in the uterus, but its free prototypes struggled each for itself in a world full of enemies, full of eliminating agencies. How many parts, therefore, have become useless to the embryo, which were useful to the prototypes! How vast is the field in which retrogression has worked! Is it any wonder, then, that the ontogeny of man is only a vague recapitulation of his phylogeny?

Reversion, then, is the necessary complement of evolution, and without it there could be no evolution, except of the simplest kind. Without reversion there could be no planing away of the numberless useless variations which occur during, and especially at the end of the ontogeny, nor of all those structures, which, though useful during some part of the phylogeny, became useless later. Without reversion, therefore, a species would soon become so burdened with useless variations and structures as to be incapable of existence. Reversed Selection could not cause the elimination of all these useless and burdensome characters; for no matter how burdensome, and, therefore, worse than useless, they are in the aggregate, separately they are so little burdensome that Reversed Selection could not act. It could not act on them in the aggregate, for this would mean that in some individuals they would be present *en masse*, whereas they would be absent *en masse* in others; and this, of course, we know is not the case. Moreover Reversed Selection causes a retracement, not a lapsing of characters. It therefore works at a double disadvantage as compared with ordinary Natural Selection, and, as a consequence, can effect comparatively little. No extensive examples of such retracement are in fact known to us in Nature. Again, without retrogression, the recapitulation of the phylogeny in the ontogeny would be impossible, and, for this reason once again, evolution would be impossible. For, were there no retrogression, the prototypes of the phylogeny would necessarily be

reproduced exactly in the ontogeny, and then the latter would be as elaborate, and almost as lengthy as regards time, as the former. Moreover, the prototypes of the phylogeny could not exist in the enormously changed environment of the ontogeny. How, for instance, could a gill-breathing animal, or any of the higher forms which intervene between them and man, exist in the uterus, in which alone can exist those dim representations of the phylogeny that constitute man's ontogeny?

It is this great change of environment, this close protection of the individual in the uterus and afterwards, which has rendered possible the evolution of man and the other higher animals. Opportunity has thus been afforded for retrogression to plane away innumerable characters which had become useless. The ontogeny has thereby been straightened, shortened, and simplified, and the evolution of new characters, useful in the new environment, has become possible. Thus, for instance, have been rendered possible the higher characters of man, for even after birth he is closely protected, and, therefore, even in that portion of the ontogeny which intervenes between the infant and the adult has there been much retrogression. Consider how feeble and helpless is the infant after birth; but its prototypes of the phylogeny fought for their own existences. The infant can digest scarcely anything but milk, and its jaws are very feeble. Its prototype must have had much wider powers of digestion. Perhaps more remarkable than anything else is the retrogression of instinct in man. I have dealt at length with this question elsewhere, and have not space for it here; but consider how helpless is the infant at birth, how extremely incapable, as compared to young insects, for instance, of adapting itself, of its own initiative, to the environment. Later on it acquires all kinds of knowledge and ways of thinking and acting, which serve as a superior substitute for instinct. But meanwhile the mother's protection, which has rendered possible this acquirement, has rendered useless also the instincts of its prototypes, which have therefore lapsed. Hence the retrogression of instinct in man. By it his mental ontogeny is shortened and simplified, just as by the retrogression of bodily parts his physical ontogeny is shortened.

In the foregoing I have spoken of characters lapsing in orderly succession, the last first, the earlier later. But it seems to me probable that earlier characters may sometimes lapse before the later. This may happen when some parts of the phylogeny, and consequently of the ontogeny, are not direct, but form a loop, so to speak. The omission of the loop would straighten, and therefore shorten, the ontogeny, and considering how condensed is the latter, I believe this must often occur.

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V

The Movement of Diatoms

THIS paper was suggested by a perusal of Robert Lauterborn's "Untersuchungen über Bau, Kernteilung und Bewegung des Diatomeen." It would be impossible to do justice to a work in which the letterpress occupies 165 quarto pages, within the limits of a single article. The present communication, therefore, will deal mainly with the movement of Diatoms, omitting, for the present, any reference to the interesting chapters on the protoplasm and its inclusions, and on nuclear and cell division—a portion of the work well worth separate treatment. For ready comprehension, however, the subject must be introduced by an account of the structure of the frustule in *Pinnularia* and *Surrirella*.

Structure of the Frustule in Pinnularia major.—The general appearance of *Pinnularia* is sufficiently well known to render any special description superfluous. The chief features visible in a surface view of the frustule, as shown in Plate III., Fig. 7, are (1) the presence of a median longitudinal undulating line, the 'raphe,' with terminal and central nodes, and (2) a double row of transverse markings, the so-called 'striae' or 'costae.' The true significance of these structures is brought out clearly by Lauterborn.

The raphe had previously been interpreted by the majority of investigators as an open cleft placing the interior of the cell in direct communication with the surrounding medium, and Pfitzer also sought to justify this view on various grounds. Flögel, however, contended that the cleft was closed on its inner side by a thin membrane. In order to settle this point Lauterborn carefully examined empty frustules of *Pinnularia major*, and also transverse sections (2-3 μ) of examples fixed with chromosmium and stained with haematoxylin. One of these sections, from the region between the central mass of protoplasm and the extremity of the cell, is shown in Fig. 1. The cell-wall exhibits a marked thickening in the neighbourhood of the raphe, which runs as a narrow angular cleft from exterior to interior without any distinguishable trace of an inner limiting membrane. The appearance of this cleft varies considerably according to the position of the section in the series (see Figs. 2 and 3), being at some points simply oblique, at others variously bent, and this irregularity in shape explains why the aspect of the raphe alters with a change of focus. In some sections (Fig. 3),

the walls of the cleft approach one another very closely in such a manner as to simulate the appearance of an internal closure. In those examples in which the cleft, seen in transverse section, has an angular course, O. Müller considers it probable that the central portion is closed during life by the middle lamina, in which case two canals would persist, one running along the outer surface, the other along the inner surface of the cell-wall, but Lauterborn has not observed such a closure, nor does he consider its occurrence probable. This is of some importance, since Müller postulated the presence of such canals in his theory of the movements of diatoms.

As to the transverse markings (Riefen) of the frustule, Lauterborn's results are entirely in agreement with those of Flögel, who had previously described the appearances as being, in reality, chambers hollowed out in the substance of the frustule, and communicating by a tolerably wide opening with the interior of the cell. These features are well shown in Fig. 1, where the plane of section passes through four of these chambers, and Fig. 2, which is an outline drawing of a section passing between two chambers in one valve of the frustule. These drawings also illustrate the way in which the two valves are united by the overlapping of their free edges.

The interior of the cell is lined during life by a layer of protoplasm, thinner laterally than elsewhere, and, in the middle of the cell, forming a transverse bridge-like mass containing the nucleus. On each side of this central portion the peripheral protoplasm surrounds a large vacuole filled with cell-sap. This condition of things suggested to Lauterborn a possible explanation of its meaning, which, although purely hypothetical, is worth consideration.

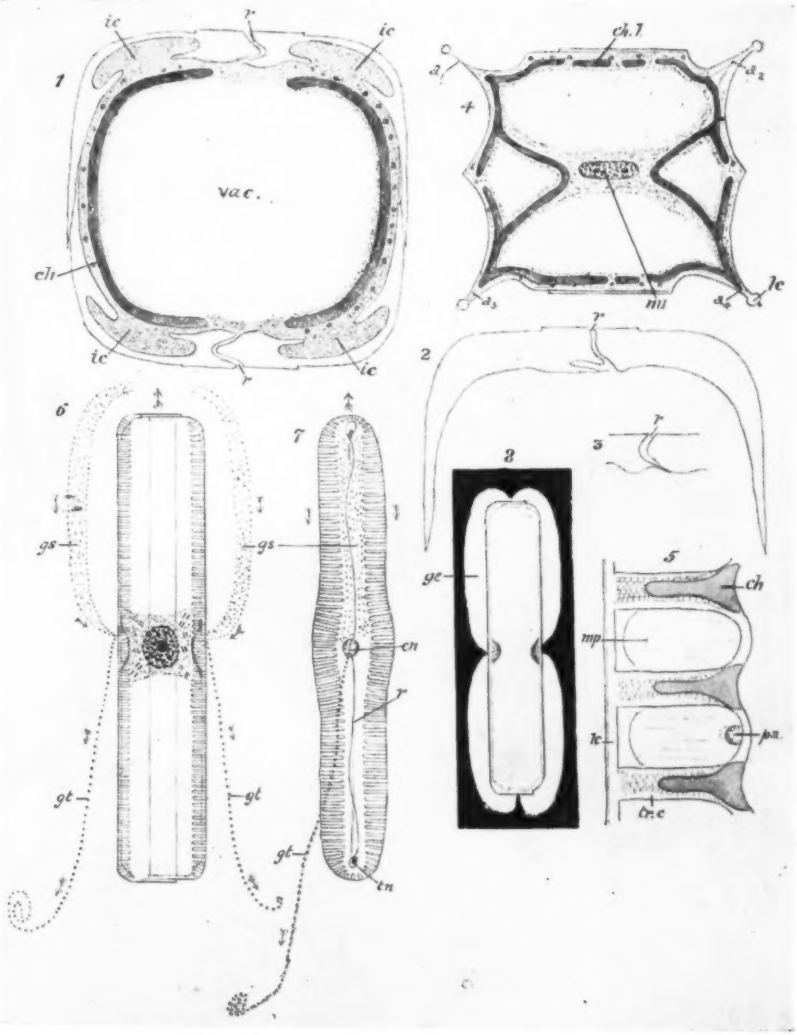
The researches of O. Müller have shown that the protoplasm of *Pinnularia* is subjected to a very considerable osmotic pressure (4 to 5 atmospheres), manifested with equal intensity in all directions. Now pressure in the direction of the lateral walls would result in pressing the overlapping elements closer together, but when exerted at right angles to this, in the direction of the raphe, there would be a tendency to force the two halves of the frustule apart, if the frictional resistance of the overlapping elements was not sufficient to maintain equilibrium. But, as we have already seen, on each side of the raphe internally a great number of transverse chambers are placed one behind the other, and filled by prolongations of the peripheral protoplasm that enter each chamber by an opening about half the diameter of its internal cavity. Lauterborn suggests that these chambers filled with abstricted portions of the protoplasm might be imagined to act as so many clamps ('Klammern') opposing a strong resistance to the force tending to separate the two halves of the frustule, and so strengthening the frictional resistance of the overlapping elements. He points out

that this hypothesis would gain in probability if it could be proved that the protoplasm was withdrawn from the chambers when the two halves of the frustule parted from one another during cell-division, a circumstance which he regards as by no means improbable. Pfitzer does not consider the hypothesis summarised above very probable, but believes that the chambers with their very thin outer walls would probably relieve the osmotic pressure; Lauterborn, however, seems justified in his contention that the two functions in no way exclude one another.

Structure of the Frustule in Surirella calcarata.—This diatom is one of the largest and most beautiful fresh-water forms. Examples measuring 0·3 mm. in length and 0·2 mm. in lateral breadth were not uncommon, so that, under favourable conditions, they could be readily perceived with the naked eye.

Seen in transverse section (Fig. 4) the outline is roughly rectangular, with two sides ('Gurtelseiten') flattened, and the other two ('Schalenseiten') more or less hollowed out on each side of a median ridge or keel. At each corner is an outstanding process (*ala*, 'Flügel'). These processes vary in appearance with the plane of section (see description of Plate) and three aspects are shown in the figure, combining features exhibited by different sections. The drawing also illustrates the complicated character of the chromatophores, and their relation to the central bridge-like mass of protoplasm containing the nucleus. When viewed in its entirety, and with one of the lateral surfaces (flattened sides in section) turned towards the observer, the diatom presents the appearance of a more or less broad wedge, the median portion occupied by the lobed chromatophores (superficial focus) and flanked on each side by the *alae* which are now seen to consist of a number of parallel transverse canals connected at their extremities by a longitudinal canal, and separated from each other by tolerably wide interspaces. In a surface view ('Schalenseite') the contour of the frustule is almost lanceolate with a median longitudinal ridge produced at either end into a spur-like process, the anterior one being the largest.

To return to the alae, Figure 5 shows a portion of one of these wing-like processes viewed from the surface. Along the edge, for its whole extent, passes a rather narrow canal ('Flügelrandkanal' or 'Flügellängskanal') a connection being established between this and the interior of the cell by numerous short transverse canals. Internally, these latter commence as tolerably wide tubes of rounded section; towards the longitudinal canal (*lc.*), however, they steadily diminish becoming more elliptical in section. The transverse canals are separated from one another by **U**-shaped intermediate pieces, formed of apposed portions of the cell-wall, whilst the spots where



THE STRUCTURE OF DIATOMS, AFTER LAUTERBORN.

FIG. 1.—Transverse section through *Pinnularia major* in the region between the extremity of the cell and the median mass of protoplasm (combination figure). *Ch.*, chromatophore; *ic.*, chambers ('Riefen') on inner surface of cell-wall; *r.*, raphe; *vac.*, vacuole.

FIG. 2.—Transverse section through one valve of the frustule of *P. major*. The section passes between two of the chambers, so that the contour of the cell-wall is unbroken except at *r*, the raphe.

FIG. 3.—Transverse section through the raphe, from another section, showing an apparent closure of the cleft internally.

FIG. 4.—Median transverse section through *Surirella calcarata* (combination figure). At a_1 the section passes through one of the intermediate pieces in the ala, at a_2 it just encroaches upon one of the transverse canals; a_3, a_4, two transverse canals bisected; *chl.*, superficial lobular process of chromatophore; *lc.*, longitudinal canal; *nu.*, nucleus.

FIG. 5.—Surface view of a portion of one of the alae of *S. calcarata*. *Ch.*, process of chromatophore surrounded by protoplasm; *lc.*, longitudinal canal; *mp.*, intermediate piece; *pa.*, unicellular alga; *trc.*, transverse canal.

FIG. 6.—Lateral view of *Pinnularia major* moving in an emulsion of Indian Ink.

FIG. 7.—Surface view of the same. *Cn.*, central node; *gs.*, anterior granule streams; *gt.*, gelatinous threads with adhering granules; *r.*, raphe; *tn.*, terminal node.

FIG. 8.—Resting example of *Pinnularia* in concentrated emulsion of Indian ink (lateral view); *ge.*, gelatinous envelope.

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they remain separate give rise to the canals. Consequently, each of these intermediate pieces forms a niche-like depression between two projecting transverse canals and a corresponding piece of the longitudinal canal. Lobular processes of the chromatophores project into the transverse canals and are always enveloped by protoplasm, which passes from their extremities towards the outer wall as a strand, usually undivided. These strands exhibit both longitudinal and transverse fibrillation during life, and, as a rule, appear to consist of five or six longitudinal rows of cellular compartments.

The longitudinal canal was described by Flögel as closed externally, but Lauterborn's sections show that it is interrupted along its outer edge by a very narrow cleft, thus placing the interior of the cell in direct communication with the surrounding medium (Fig. 4). This cleft is not only visible in sections through the frustule, but can also be made out in a surface view when one of the alae is viewed vertically, parallel to the direction of the transverse canals. Yet another circumstance, not taken into account by Flögel, points to a breach of continuity in the cell-wall at this spot. In the living *Surirella*, foreign bodies (*e.g.* particles of Indian ink, sand, small diatoms,) are readily observable adhering along the edges of the alae, where they are moved briskly to and fro in the same manner as along the raphe of *Pinnularia*, *Navicula*, etc., a circumstance very difficult to explain if the cell-wall were in reality unbroken.

Most of the *Surirella* were infested externally by a small alga belonging to the Cyanophyceae (*Chroococcus* sp.). The small blue-green spherical cells always occurred in a definite situation, viz., in the niche-like intermediate pieces of the alae, only a single alga being present, as a rule, in each cavity (Fig. 5 *pa.*). More than once, examples of *Surirella* were noticed where all the intermediate pieces of the alae sheltered these little lodgers. It is pointed out that the advantage may well lie exclusively on the side of the *Chroococcus*, a possible explanation being that the alga, seated on the diatom in the very fine and easily disturbed mud, suffers less interruption of the assimilative processes than if it were free in the mud, because the diatoms, if buried by the movements of fishes or creeping molluscs, soon work their way up again to the surface and so to the light. In this connection it may be mentioned that Gastrotricha, of the genera *Chaetonotus* and *Ichthydium*, constantly attach their eggs to the surface of large *Surirellae*.

The Movement of Diatoms.—Various hypotheses have been advanced to account for the characteristic movements more or less familiar to every student of the Diatomaceae. Amongst earlier investigators, M. Schultze held that protoplasm, protruded through the slit-like raphe, served to set the cell in motion, and this view was maintained by Pfitzer and Engelmann. On the other hand Nägeli,

Siebold, Dippel, Borscow, and particularly Mereschkowsky, regarded the cell movement as the result of an osmotic phenomenon, supposing water to be imbibed at the anterior end of the diatom and expelled with greater force at the hinder extremity, the recoil serving to propel the cell onwards.

Bütschli and Lauterborn worked together at the subject, and the former published a preliminary account of their researches, which was adversely criticised by O. Müller. A reply from Lauterborn drew a further communication from Müller, and this is dealt with in the work under consideration, where the Bütschli-Lauterborn observations and inferences are given at length, with a detailed criticism of Müller's objections and theory. The observations described and illustrated by Lauterborn first claim attention, and will be best given as nearly as possible in his own words.

When large examples of *Pinnularia* are brought into a concentrated emulsion of Indian ink,[1] the majority of the diatoms present at the first glance a very striking appearance, each being surrounded by a broad, bright, and sharply defined border, within which the smaller granules of Indian ink do not penetrate. In a surface view this halo usually follows the contour of the frustule at a distance equal to about half the width of the valve; but when the lateral aspect of the diatom is turned towards the observer, this clear border is seen to be interrupted in a symmetrical manner at both ends of the cell, and also in the vicinity of both central nodes (Fig. 8); here the granules of Indian ink approach close to the cell-wall.

The appearance just described is interpreted by Lauterborn as pointing to the existence of an enveloping layer of hyaline jelly, so remarkably transparent, and possessing a refractive index corresponding so closely with that of the surrounding water, as to be completely invisible in clear water, even when examined with the best lenses.[2] Müller denied the general presence of a gelatinous envelope in the sense advocated by Bütschli and Lauterborn, stating that the clear border only appeared after a long sojourn in the Indian ink emulsion, and that it was absent in the living but completely motionless cell. He, however, subsequently admitted the existence of a gelatinous envelope. In answer to Müller's objections on this point, Lauterborn asserts that the hyaline border becomes visible as soon as the diatoms are brought into the emulsion, and also states that he has often seen *Pinnulariae*, surrounded by the transparent envelope, remain for hours without the slightest movement.

The presence of a peculiar and characteristic streaming move-

[1] Lauterborn recommends that the Indian ink should be rubbed up in the water in which the diatoms have been cultivated, and also that specimens from old cultures should not be employed in studying movement.

[2] A gelatinous envelope of similar transparency has been observed in a pelagic form of *Cyclotella comta*.

ment in *Pinnularia* is next described, and will be found illustrated in figs. 6 and 7. It can be seen to greatest advantage in a lateral view of the diatom in Indian ink emulsion (fig. 6). Small granules of Indian ink, set in motion in the region of the anterior[1] terminal nodes, move outwards, and then flow, at some distance from the surface of the cell, as far as the central node of the raphe. The granules borne along by these anterior streams (*gs.*) are not closely packed, and exhibit a more or less active independent movement amongst themselves. Arrived at the middle of the cell the streams of granules turn sharply towards the openings of the central nodes, forming there larger or smaller accumulations, and from these points arise granular threads (*gt.*) which always run obliquely backwards, forming an acute angle with the surface of the frustule. In watching the formation of these granular filaments, Lauterborn observes that the process strongly suggests the forcible expulsion, by fits and starts, from the central node, of a gelatinous thread, to which a single row of granules adheres. This view is strengthened by the circumstance that the granules at the moment of union with the thread at once lose their active molecular movement perceptible whilst within range of the anterior streams of granules, also that the granules are all moved backwards intermittently with the same velocity, as the thread lengthens, suggesting the presence of a common substratum binding all of them together. It often happens that the series of granules is interrupted at some point, nevertheless the direction and intensity of motion in the granules behind such a gap correspond with those in front of it, and this circumstance appears to offer additional evidence of the presence of a common bond in the shape of a gelatinous thread, absolutely invisible in clear water until brought into view by the adherence to it of foreign bodies. These threads, which lengthen as the diatom moves onward, may be five or six times as long as the frustule.

The phenomena described above are best observed when the diatom is lying on its side, as in fig. 6 ; but in a surface view (fig. 7) the streams of granules along the raphe on both sides of the frustule can be demonstrated by altering the focus. Occasionally the granule stream and thread on one side of a diatom move in the opposite direction to those on the other side, so that the two streams neutralise one another to a certain extent, and sometimes only one granular thread is developed.

In a paper published in 1891 Bütschli considered it highly probable that a causal connection existed between the backward prolongation of the granule threads and the local movements of the diatom, stating that both he and Lauterborn were disposed to

[1] The terms 'anterior' and 'posterior' are here used with reference to the direction of movement of the diatom.

regard these movements as due to a copious production of adhesive jelly ejected rapidly and with a certain force from the nodes of the frustule. Whether the extremity of the gelatinous thread struck the substratum on which the diatom rested or only encountered the resistance of the water, the effect would be the same, viz., to drive the cell in the opposite direction.

In opposition to this view O. Müller explained the phenomena as caused by a stream of cytoplasm propelled through the anterior terminal nodes into the external cleft of the raphe (which, it will be remembered, he supposed to be closed by a median lamina), and there moved towards the centre, flowing back into the interior of the cell through the canal of the central node. The stream, projecting laterally from the cleft, swept with it the suspended granules in the neighbouring layer of water, bearing them towards the narrow central canal. Here a congestion and accumulation of the cytoplasm would occur and, as the effect of the stream on the granules ceased, the latter would collect more or less, become agglutinated by the dammed-up protoplasm, and subsequently displaced backwards. So, according to Müller, the thread originated, and as the cytoplasmic stream moved intermittently, the thread would likewise elongate intermittently, and appear as if ejected from the canal of the central node. The essential feature of Müller's hypothesis lies in the view that the material extruded from the interior of the cell is true protoplasm; a view he still maintains in a later communication noticed in a postscript to Lauterborn's work.

Lauterborn argues at great length against the validity of such a conclusion, pointing out first of all that the invisibility of the extruded material under ordinary conditions militates against the protoplasmic theory, since streaming protoplasm is always recognisable as such without difficulty. It is true that Müller cited Schultze in support of the contention that under certain circumstances the presence of true plasma-streams might escape detection, but in the special cases mentioned by Schultze (*Gromia*, *Difflugia*) the contour of the pseudopodia was always clearly defined. Further, it is necessary to remember that protoplasm, which when Schultze wrote (1865) might be described as 'hyaline' or 'structureless,' is so no longer, thanks to improved optical appliances, and there is no ground for supposing that, in cases where granules are moved along by the pseudopodia of rhizopods, the protoplasm ever resembles the streaming substance in *Pinnularia*. Lauterborn also lays stress upon the fact that the collections of cytoplasm normally occurring at the poles of the *Pinnularia* cell exhibit a beautifully defined reticular structure during life, and that it is in these regions that Müller's streams of cytoplasm originate. To imagine that the cytoplasm is completely changed in passing through the polar cleft

to the exterior, so that no structural feature can then be detected even by the best apochromatic objectives, would seem highly improbable to say the least. The formation of the thread-like prolongations originating at the central node would, on Müller's hypothesis, appear to involve a considerable waste of living substance during prolonged movements ;[1] this could scarcely be brought into harmony with the economy of a single cell, and no proof is given in support of his assertion that the main streams of cytoplasm return to the interior of the cell by way of the canal of the central node. On the whole, according to Lauterborn, everything tends to show that the main streams (*gs.*) consist throughout of jelly which is driven out at the ends of the cell through the openings of the terminal nodes (especially the 'crescentic polar clefts' of Müller), moves in the raphe towards the centre, and projects laterally over it to some extent. The fact that larger fragments of Indian ink or granules of carmine sunk in this jelly are carried in it along the raphe towards the centre proves that the entire hyaline border is actually in motion throughout, but the precise mode of formation of the gelatinous threads and their relation to the rest of the streaming substance is acknowledged to be obscure.

In contrast to the conditions existing in some species of the genus *Pinnularia*, there are other diatoms, *e.g. Pinnularia oblonga* and members of the genera *Navicula*, *Pleurosigma*, and *Nitzschia*, in which a gelatinous envelope and thread-like prolongations are apparently wanting. In these, the small grains of Indian ink come into contact with the siliceous frustule, and are often moved actively about close to the raphe, as described by numerous observers. So far, the most careful examination has failed to show anything projecting from the raphe, and it would therefore seem that the substance which causes the movements of the foreign bodies, moves within the fissures of the cell-wall, whether occurring in the alae of *Surirella*, the keel of *Nitzschia*, or elsewhere. Lauterborn goes at length into the question whether a substance streaming within a narrow cleft, and so touching the surrounding medium only with a narrow linear portion of its surface, could effect the locomotion of the entire cell, and concludes that a sufficiently powerful force might be developed to overcome the frictional resistance of the surrounding water, pointing out that a somewhat similar principle (the so-called 'hydraulic reaction') has been successfully employed to propel large ships.

Owing to the impossibility of examining the substance presumed to be streaming in the raphe, one cannot say whether it is protoplasm or a gelatinous material, and it must be conceded that the

[1] Müller has since suggested that these prolongations may consist of granules only (smoke-streak appearance), but a connecting substance seems necessary in order to explain certain features observed by Lauterborn.

same mechanical effect would be produced in either case. Lauterborn is content to describe it as a viscous substance, though he apparently inclines to the belief that it corresponds to the jelly already described in *Pinnularia*, and believes that corroborative evidence for this view is furnished by an examination of *Surrirella*. The structure of the alae in *S. calcarata*, with the arrangement of the longitudinal and transverse canals have been already described. The displacement of foreign bodies along the clefts in the longitudinal canals has been long known, and it is pointed out that, although the protoplasmic strands surrounding the chromatophores in the transverse canals exhibit, in the living diatom, a well-marked reticular structure, the contents of the longitudinal canals are perfectly hyaline and can only be brought into view by staining. Thionin and methyl violet were used, after which the substance in question appeared as a granular contracted string, from which the plasma of the transverse canals was generally separated.

With reference to the capability of diatoms to produce considerable masses of jelly under certain circumstances, it is pointed out that there are a considerable number of forms in which single cells are united into colonies of variable shape by gelatinous material (*Encyonema*, *Schizonema*, *Mastogloia*, etc.). In other cases the cells are borne upon long gelatinous stalks, as in *Achnanthes* and *Gomphonema*, whilst, in auxospore formation, numerous diatoms, generally free from jelly, are known to secrete it very profusely. In places where diatoms are massed together, covering the mud on which they rest with a brown scum, it is only necessary to take a piece of the latter between the fingers in order to appreciate its slimy nature, due to the production of jelly by the living cells. A movement by the aid of a gelatinous material produced by the living cell is not confined to the Diatomaceae, but is found in other forms, both animal and vegetable. Desmids, *e.g. Closterium*, secrete a gelatinous thread by the help of which the cells are able to raise themselves upon the glass walls of the culture vessels. In the form mentioned, the thread emerges through pores at the extremity of the cell, and can only be demonstrated by staining, or by the employment of an emulsion of Indian ink.

Again, in the case of *Oscillaria*, Lauterborn describes and figures the adhesion of foreign bodies to, and their movement in spiral paths along, the algal threads. As the *Oscillaria* moves forwards, a bright streak appears at the hinder end (visible in Indian ink, or by staining) and lengthens as the *Oscillaria* advances; it is apparently to be regarded as consisting of a jelly-like substance separated on the surface of the algal thread and drawn backwards in a spiral manner. A very similar mode of progression is also met with in the Gregarines; these, according to Schewiakoff, also produce a long glutinous track,

which can be brought into view by the methods alluded to above. His illustrations show how great is the resemblance to the corresponding phenomena in the Desmidieae and *Oscillaria*.

In concluding the part of his work dealing with the movement of diatoms, Lauterborn briefly criticises a theory advanced by Hauptfleisch in 1895. In opposition to other observers Hauptfleisch would locate the organ of locomotion in *Pinnularia* in protoplasmic threads issuing from pores on the longitudinal edges of the diatom. These pores, however, are apparently non-existent, and both Lauterborn and Müller agree in considering the protoplasmic threads to be merely contracted portions of the plasmatic cell-body occupying the inner chambers of the frustule.[1]

Methods of Collecting and Preserving Diatoms.—In collecting, a spoon attached to a stick was employed for skimming the brown diatomaceous ooze off the surface of the mud, whilst in the case of forms occurring at greater depths, *e.g. Surirella*, a small drag-net was found useful for bringing samples of mud to the surface. This latter was placed with water in shallow glass vessels sheltered from direct sunlight, and after resting for about twelve hours the diatoms appeared in masses on the surface of the mud, whence they were readily transferred by means of a pipette to the fixing-fluid.

Among fixing reagents, Flemming's chromo-aceto-osmic acid, and sublimate in either water or alcohol solutions, demonstrated the most delicate structural features of the nucleus and cytoplasm during division. Picro-sulphuric acid followed by a haematoxylin stain gave excellent pictures of the chromatic elements of the nucleus. A 1% osmic acid solution served, in unstained preparations, to bring out the arrangement of the cytoplasm, the chromatophores, and other inclusions in the cell. A 45% solution of iodic alcohol is recommended for the study of the so-called 'red granules' of Bütschli, which stain exceptionally well after fixing by this method.

Only large forms could be removed individually under the dissecting microscope and by means of a capillary tube; the smallest forms were taken up in the mass by a pipette and at once placed in a tube containing the fixing solution. Here they remained for about fifteen minutes, after which, the fixing fluid being decanted off, they were well washed in water, and afterwards passed, through alcohols of increasing strength, into absolute alcohol, where they remained until all the colouring matter of the chromatophores was extracted, and any oil globules removed. The addition of a drop or two of sulphuric ether and the application of a moderate amount of heat facilitated this process. Afterwards, the material was passed through alcohols of decreasing strength into distilled water, in readiness for staining.

The most useful stain was a weak solution of Delafield's haematoxylin, but it was necessary to control the process under the microscope in order to prevent overstaining. Alum- and borax-carmine were also tried, but with decidedly inferior results. Safranin was useful

[1] O. Müller, in a paper which I have not yet read (*Ber. Deutsch. Bot. Gesell.*, xv. (1897), pp. 70-78), seems now to regard movement as taking place by means of currents of a mucilaginous substance projecting from the raphe. See *Jour. Roy. Micro. Soc.*, 1897, p. 234.

for demonstrating the centrosome and nucleoli in specially prepared material. When stained, the specimens were passed successively through 35%, 70%, 95%, and absolute alcohol into oil of cloves for clearing purposes, and finally mounted in dammar. The alcohol baths must be changed very gradually both when fixing and staining, otherwise distortions of the protoplasm are certain to occur. Isolation of the nucleus in *Surirella* could be accomplished by placing the stained specimen in dammar under a cover-glass provided with wax feet and pressing this down until the frustule began to gape. Then, by gently and persistently tapping the cover-glass with a needle, the nucleus could often be completely freed for examination.

It was possible to stain the diatoms to a certain extent during life in a very weak solution of methylene blue (1 in 100,000), in which they would live for days. If transferred from this to a stronger solution (0·01%), some of the cell contents were stained in a very characteristic manner; but so soon as the nucleus began to take up the stain, it was a sure sign that the vitality of the cell was on the wane, although the diatom might continue to move slowly for a time. In no case was observation of living specimens omitted, for a whole succession of phenomena could only be adequately studied in this way, and such observations possess special value for checking the results obtained after the use of reagents. A Seibert apochromatic objective of 2 mm. focal length was usually employed, in combination with a No. 12 ocular, giving a magnification of about 1200.

Town Museum, Leicester. F. R. Rowley.

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SOME NEW BOOKS

The Hypnotising of Animals

Beiträge zur Physiologie des Centralnervensystems I. Die sogenannte Hypnose der Thieren. By Max Verworn. 8vo, pp. iv+92, with 18 text-figures. Jena: G. Fischer, 1898. Price M. 2.50.

Professor Verworn is to be congratulated upon the production of the above work. The subject-matter is one of very great interest; it is set forth in such lucid and agreeable style as to make the book excellent reading, whilst the method of treatment adopted by the author gives the treatise a high value, and renders it an experimental contribution to the physiology of the nervous system based upon original lines. This will be made clear by a short sketch of the scope and aim of the work.

The so-called 'hypnosis' of animals is a well-known state of immobility resembling on superficial examination the condition of hypnotic trance which can be produced in man. A dissimilarity between the 'state' in lower animals and that in the hypnotised human subject is however present at the very outset, the means of production being different in the two cases. In man those mental states which are implied by the term 'suggestion' play an important part as precursors of the condition; but in lower animals the essential agency is the maintenance of the body by external force in an abnormal position. Examples are given in the book of the state of immobility into which animals of very different types may fall under these conditions. The original part of the work is the demonstration by Professor Verworn that the immobile state in these animals has two prominent characteristics which are significant of the physiological factors concerned in its production. These are, first a special form of activity in the muscles, and, secondly, a peculiar condition of inactivity of the cerebral hemispheres.

With regard to the muscles the author shows that persistent reflex tonus is present, and is most marked in such groups of muscles as the animal would utilise for regaining its normal position of bodily equilibrium. If, for example, the state has been caused in the guinea-pig by keeping it upon its back, then the muscles concerned are those the animal employs for turning into the customary attitude. When held by force in the abnormal position the animal vainly contracts these muscles for this purpose, and the immobile state commences with the sudden cessation of the nervous outflow from the higher centres producing these vain efforts; this is immediately succeeded by a set tonus of the muscular groups especially those involved in the previous efforts. If the restraint has been such as to affect one group more than others, then the subsequent tonicity or 'contracture' is particularly prominent in this group. The state thus differs from sleep in the disposition of the limbs, trunk, head, eyes, &c., which may be made to

assume all manner of different appearances by appropriate previous manipulation, so that the attitude can be of a most bizarre kind. During the continuance of the immobile state the organs of sensation, peripheral and central, show no evidence of any alteration, and the animal thus appears to be conscious of the various sensations produced by external impressions, but if these are sufficiently intense to evoke an efferent discharge from the cerebral hemispheres, the state at once ends and recovery takes place. This recovery is shown to be ushered in by augmented contractions of those muscular groups which are in the state of more pronounced contracture; at first these are ineffectual to alter the position of the whole body, but with their repetition the contracture subsides and the alteration is effected. The author has not been able to confirm Danilewsky's observation that during the state reflex excitability is lowered; he regards the previous evidence of such lowering as due to the peculiar condition of the muscles, which owing to contracture are incapable of adequate response to central nervous discharge. Lowered reflex excitability only occurs when an animal has been manipulated many times in rapid succession, in which case central fatigue manifests itself and the contracture is correspondingly diminished. It will be seen from the foregoing description that the characteristic condition of the muscles, *i.e.* tonicity, disproves the existence of any inhibition of lower neuro-muscular mechanisms during the state; on the contrary, the lower centres—cerebellum, medulla, &c., being released from cerebral control, now discharge a continuous stream of nervous impulses such as occurs in the decerebrate mammal, and this produces the marked decerebrate rigidity described by Sherrington, Horsley, and others.

With regard to the second factor in the production of the state, it is shown that the cessation of the discharge of impulses from the cerebral cortex is a complete one. Such complete cessation must exist when the cerebral hemispheres have been previously removed; in such animals the immobile state can be produced with great ease, and is always of a most prolonged type, whilst the stimulating agencies necessary to produce recovery, have to be of an intense character. In the intact animal the author considers that the sudden cessation of cerebral discharge cannot be explained as due to the lack of stimulation of the motor areas existing in the cerebral cortex since the physiological avenues for sensation are unaffected. He believes that the paralysis of these centres is brought about by a sudden inhibition due to the activity of special parts of the nervous system or to special conditions of the centres themselves. His conception of such conditions is set forth at some length in the concluding chapters of the work, and is framed upon the lines of Hering's well-known views of the physiological states of activity and repose.

The whole work affords most striking instances of the opposite rôles to be assigned to cerebral and to lower centres respectively, hence, its perusal may be confidently recommended to all those whose special interests lie in physiology or neurology. But apart from the obvious scientific value of the book, the earlier chapters contain a description of phenomena which will enable those interested in hypnotism to realise what the so-called hypnotic state of an animal is like, and

how it can be produced. A further interest is given by the excellent account of the observations and views of other writers upon the subject, from Pater Kircher who described the *experimentum mirabile* in 1646 down to the present day, and the value of this is enhanced by the well-selected woodcuts with which the account is illustrated. The whole forms a work which fully sustains the well-merited reputation Professor Verworn has derived from his previous publications.

FRANCIS GOTCH.

MR BEDDARD ON BIRDS

THE STRUCTURE AND CLASSIFICATION OF BIRDS. By Frank E. Beddard. 8vo, pp. xx + 548. London: Longmans, Green & Co. 1898. Price 21s.

THIS volume comes to us as a promise long delayed, and conjures up visions of men whom many of us have never seen, but who yet live in their works, and will live, as long as ornitholgy, in its deepest and truest sense, continues to be studied amongst us. It represents three occupants of the Prosectorial Chair of the Zoological Society of London. It was begun by Garrod, and contemplated by Forbes, but before either could come within a measureable distance of its completion they were summoned by death, and another entered into their labours. It was for Mr Beddard, the present Prosector, to realise what they had always hoped to do, and than him a more fitting person could not be found. Perhaps the highest praise we can give will be to say that the result is worthy of all three, and that it would have met with the entire approval of either of his predecessors.

Although the work of Garrod and Forbes has been largely drawn upon, Mr Beddard has incorporated the essence of all that is best of his own work and that of his contemporaries.

He divides his book into two parts—(1) General Structure; (2) Classification. Under the first head, among other subjects, he deals with the coelom, convolutions of the intestines, and the syrinx in a full and able manner, and these pages will be found to contain a vast amount of most valuable matter. It surprises us, however, to find that no figure is given of the passerine syrinx.

The possession of feathers, an ambiens muscle, and an oil-gland are the characters enumerated by Mr Beddard as peculiarly avian, the two last having been acquired within the class. About the first and last of these no one has ever expressed any doubt, but there are many who have regarded the ambiens as reptilian in origin. The absence of an oil-gland, it is pointed out, may be a primitive and not pseudo-primitive character. The Struthiones are defined as birds in which the gland is absent, though a page or two further on it is stated to be present in *Apteryx*, and correctly so. The primitive feathering of birds Mr Beddard thinks was in the form of downs. "The persistence of downs, therefore, in this hypothesis is so far a primitive character, and the greater the persistence the more primitive the bird." We certainly doubt whether this is not proving too much. If this is true, then the Anseres, Accipitres, Charadrii, and Ralli, for example, would be more primitive than the Pico-passeres, or the Struthiones and Galli. Again "the fact that the contour feathers are frequently preceded by downs" is not convincing proof that this was the primitive covering of birds. Inasmuch as the down-feathers

which precede the contour-feathers are quite distinct from those which form the down-feathers proper. The former in the ducks, the fowls, megapodes, and tinamous, for instance, probably much more nearly represent the primitive clothing, and are in the nature of semiplumae, which as Garrod and the present writer have shown, are degenerate contour-feathers. We incline to agree with Gadow and to hold that the absence of down-feathers is primitive.

The presence of teeth is undoubtedly primitive. "Arrested dental papillae" are instanced as occurring in *Phytotoma rara* and in the merganser. The present writer has figured and drawn attention to similar structures in the tinamou and *Opisthocomus*.

In describing the skull, no mention is made of the parasphenoid. We are told that the "base of the brain-case is protected by a large basitemporal which has sometimes (*e.g.* Apteryx) a long rostrum in front." For sometimes 'always' should surely be substituted. The description of the hyoid is unintelligible. As regards the pelvis, we entirely agree with Mr Beddard in favouring the view that the pelvis of Aves most nearly resembles that of the dinosaurs. We further agree with him in regarding the pectineal process as the equivalent of the forwardly directed process of the dinosaurian pubis, and the backward process as the homologue of the pubis proper in the two groups.

The classification of birds is a subject of peculiar difficulty, and no two ornithologists can be persuaded to think alike on this matter. Its importance in Mr Beddard's estimation can be gathered from the fact that he has devoted two-thirds of his book to this question. And in these pages will be found some extremely valuable and helpful suggestions, which will afford food for reflection for a long time to come. He divides the class into two sub-classes, 'Ornithurae' and 'Saururae,' corresponding to the Neornithes and Archaeornithes of Gadow. The Ornithurae are further divided into 'Anomalogonatae' and 'Homalogonatae.' But it is unfortunate that nowhere is the latter group defined or are its boundaries fixed. As to the arrangement of the sub-orders we will only say that we should have preferred to see the Tubinares placed next the Sphenisci, the Tinami near the Galli, and the Accipitres nearer the Steganopodes and Herodiones.

Space forbids discussion of this book at greater length. Some room must be left wherein to protest against any suspicion of captious criticism or querulous fault-finding. Whatever statements we have taken exception to have been selected not as an instance of many such, but as blemishes to be removed should a second edition be called for, which is highly probable. Those who have occasion to use this book most will learn soonest to find out its sterling value, and such will best appreciate the fairness of our remarks. W. P. P.

"O THOU WONDROUS MOTHER-AGE!"

THE WONDERFUL CENTURY: ITS SUCCESSES AND ITS FAILURES. By Alfred Russel Wallace. 8vo, pp. xii + 400, with 12 folding tables and frontispiece portrait. London: Sonnenschein. 1898. Price 7s. 6d.

THIS book is an appreciation of the nineteenth century, an attempt to look at it in its relations to the whole history of man as it will appear

to the historian of the future. The successes have been in an increased knowledge of the facts and governing principles of the world around us, and in the application of them to our benefit. The failures have lain chiefly in the field of social economy, in which the advance has been incommensurate with that in the region of physics.

The striking feature of the century has been the discovery and application of scientific and mechanical principles entirely unknown to previous ages; discoveries comparable to the invention of fire, of writing, of geometry, or of printing; applications that have revolutionised the mode of life of nearly all the world, bringing changes both wide and deep where change had been unknown for centuries, or even for millennia. Chief among these are the means of communication by railways, steamships, the electric telegraph, and the telephone. Then come modes of lighting, friction matches, gas light, and electric light. The knowledge of light itself, and its action on matter, with the marvellous applications to photography, the Röntgen rays and spectrum analysis, by which last our knowledge of the distant universe has been so enormously extended in so many directions. Minor mechanical inventions of a novel order are the phonograph, the typewriter, and the cycle. Among scientific theories, whose practical application, though not always so direct or obvious, has profoundly altered our ways of thought, or given us fresh mastery over matter, Mr Wallace notes the following:—The doctrine of the conservation of energy; the molecular theory of gases; the atomic theory as the foundation of modern chemistry; the uses of dust; a knowledge of meteors and the meteoritic theory of the universe (the latter perhaps not so generally accepted as to have a right to rank in the present category); the hypothesis of a glacial epoch (in which also Mr Wallace goes further than many admit); the vaster conception of the antiquity of man; the cell theory and the theory of recapitulation in embryology (where, likewise, a hint of recent criticism would not have been misplaced); the germ-theory of disease and the function of leucocytes, from which conceptions Mr Wallace, not quite fairly, separates antiseptic surgery; the use of anaesthetics; and the acceptance of the theory of organic evolution, an acceptance due chiefly to the labours of Darwin, whose "work will always be considered as one of the greatest, if not the very greatest, of the scientific achievements of the nineteenth century."

This first half, or rather, less than half, of the book is a well-balanced and thoroughly interesting review, making its chief appeal to the ordinary intelligent reader. It might have been written, perhaps not quite so well, by any competent man of science. The second section of the book, dealing with the failures of the century, could have been written only by Mr Wallace. As an expression of the convictions of an eminent naturalist and thinker on many of the most important problems of our day, it has a value by virtue of that personal element, and demands the attention of all, whether they agree with its opinions or no. The list opens curiously with a strong statement of the case for phrenology, the neglect of which is regarded as one of the chief failures; the chapter undoubtedly provokes one to a reconsideration of the subject. Of similar nature is the opposition to hypnotism and physical research, so prevalent among scientific men.

It is true that exposure of charlatan after charlatan has raised a serious prejudice against such truth as does lie in these obscure branches of knowledge, while the sensational appeal they make to the unbalanced mind of the uneducated must always lead the scientific investigator to approach them with a caution and scepticism greater even than that which he rightly applies to all subjects of his study. But to write this down as a failure is to go too far. Much has been attempted and accomplished by trained observers and professional medical men, while mention at least should have been made of the establishment of the entirely new science of psychology, from the critical and experimental study of which far more promising results have already been derived than from the not always edifying exhibitions of mediums and clairvoyants. To judge from the fact that a quarter of the book is devoted to it, vaccination is the subject on which Mr Wallace feels most strongly. The opponents of this operation can hardly say that it was accepted and enforced with unthinking speed; moreover, improvements have been and are constantly being made, and we can hardly regard the statistics here collected as applicable to the vaccination of the future. The concluding chapters deal with militarism, the treatment of criminals, concentration of capital with its corresponding increase of absolute poverty, followed by the deterioration of those brought under its influence, and the spoliation of the products of the earth, such as forests, coal, and the fertile soil. That in these respects our century is no better, and often far worse than its predecessors, is too generally admitted to need emphasis here. But, whether or no the remedies to be adopted are those suggested by Mr Wallace, we venture to believe that remedies are being sought for most earnestly by an increasing number of men and women, and that even the nineteenth century may claim more than is here allowed to it. Arbitration has made progress, the treatment of criminals has improved, co-operation has become more general, schools of forestry are held to be essential, even the much-abused Indian Government has constructed irrigation works that will be the wonder of ages to come, and, as Sir William Crookes lately told us, the chemist is prepared to refertilise our worn-out soil.

Let us not be too pessimistic. No advantage is ever gained without a corresponding disadvantage, and we cannot look for advance in all directions at once. The evils that we all deplore have been caused by those very benefits that we give thanks for, and a recognition of the evil is the first step towards its removal. As an honest attempt to look things straight in the face, Mr Wallace's book deserves a welcome from men of all opinions.

Geology made Easy

Geology for Beginners. By W. W. Watts. 8vo, pp. xviii+352, with 310 illustrations. London: Macmillan & Co. 1898. Price 2s. 6d.

This is one of the best introductions to Geology that we have ever seen. Most books of the kind are ship-wrecked on a syllabus; but Mr Watts, though planning his work on the lines of the revised syllabus of the Science and Art Department, has managed to steer safely through its narrow passages without sacrificing breadth of view or originality of treatment.

Two features of the book are prominent. One, for which the author expresses indebtedness to the late Prof. Green, is a constant appeal to actual observation and experiment, the value of which in so practical a science as geology can hardly be overrated. The other, which is indeed the natural corollary of the former, is the introduction of numerous photographs of actual sections and views of geological interest. For this Mr Watts has peculiar facilities, as secretary to the British Association Committee for collecting and preserving such photographs. On the whole these photographs are well selected, and add greatly to the attractiveness of the book. But nearly all of them suffer from the printing, and there are many which we have found in actual use to be unintelligible to the student. Figures 13 and 19, purporting to show Crinoidal and Wenlock Limestone, might be almost anything, so great is the reduction. Figures 18, 60, 88, 104, 154, 155, 296, 297, 301, are among those that would have been more effective as pen-and-ink line-drawings, such as the excellent fig. 53. The introduction of cross-country sections is to be commended; but the compression of a section across the Snowdon range or the ancient rocks of Pembrokeshire into three inches does not make its unravelment either easy or pleasurable. We rejoice to see that the very clear woodcuts of De la Beche are still available. As for the figures borrowed from Zittel and distinguished by (Z), Mr Watts doubtless knows who the true authors of most of them were and has suppressed the intelligence after due consideration. We allude to this because there is a lamentable tendency on the part of text-book writers to copy figures from other text-books, and to give credit to the copyist or compiler rather than to the original author. Thus the student is led in a mazy round and not to the fountain-head.

In style and arrangement the work is remarkably clear. General or doubtful statements receive their necessary qualification, so often omitted in elementary text-books. Errors there may be, but they are not very serious. One fault should be remedied in a future edition; that is the introduction of technical terms in the legends to figures, without any explanation in the text, or without cross-reference to such explanation if given on a later page. Ice-tables, for instance, should either have been explained, or they should not have had a half-page illustration devoted to them.

The book is a fitting celebration of Mr Watts' appointment to an assistant-professorship at Mason College, Birmingham. On its appearance, both the author and the beginner in geology are to be congratulated.

The Affinities of Animals

L'anatomie comparée des Animaux Basée sur L'embryologie. Par Louis Roule. 8vo, pp. xxvi+1972, with 1202 figures. Paris: Masson et Cie. 1898. Price 48 francs.

Mr Roule is Professor at the University of Toulouse and is well known by his works "Embryologie générale" and "Embryologie comparée," to which the two bulky tomes before us form a natural sequel. The object of the book is not to give either a systematic summary of the whole animal kingdom, a detailed account of its numerous variations of structure, or even an elaborate discussion of

selected types, but rather to show the relationships of animals and thus to manifest evolution in the Animal Kingdom. As the chief guide in determining those relationships, Prof. Roule takes embryology, although palaeontology and comparative morphology and histology are not rejected.

The Animal Kingdom is divided into sixteen branches, dealt with in the following order: Sarcodic Protozoa, Ciliate Protozoa, Mesozoa, Spongida, Hydrozoa, Scyphozoa, Plathelminthes, Nemathelminthes, Trochozoa (Rotifera; Bryozoa, Brachiopoda, Phoronidea, Sipunculidea; Mollusca; Archiannelida, Hirudinea, Chaetopoda; Pseudannelida = Sternaspids & Echiurians), Arthropoda, Chaetognatha, Peripatida, Echinoderma, Enteropneusta, Tunicata, Vertebrata. Each of these branches is dealt with under the following heads: General considerations and relations to other branches; Distribution in nature; General organisation, first of the embryo, then of the adult; Comparative account of the different body systems, as manifested in the various Classes of the Branch; Principles of classification, division into Classes, and mutual relations of the Classes; Bibliography. The second volume ends with two indices, the first to the zoological names, the second to the anatomical terms. There is no general chapter dealing with the Animal Kingdom as a whole, or with the classification adopted, since that was given in the previous works referred to above.

Seeing that the subject is one of such obscurity, and open to so wide diversity of opinion, it hardly seems worth while pointing out the paths along which we should not care to follow our professorial guide. That he has made the attempt, and that Messrs Masson have published it, is alone a reason for gratitude. For the book, though somewhat wordy, and occasionally less clear than we are accustomed to from a Frenchman, furnishes a series of very readable accounts with many suggestions of interest. Without casting any slur on embryological research, we must confess to some distrust in those who place quite as much reliance on it as does Prof. Roule. But taking it at his valuation, we fail to see how it lends support to the view that the Nautiloidea are ancestral to the Ammonoidea and the admittedly dibranchiate forms; for their embryology shows clearly that the Nautiloidea have lost an important structure, the protoconch, once possessed by them and still possessed by the other orders. We also venture to think that the known facts in the embryology of recent echinoderms afford no proof whatever that the five-rayed ancestor, which the holothurians must have had in common with the other classes, was less developed than many cystids. Prof. Roule's 'hypothetical Pentazoön' is not the most ancestral form that is shadowed forth to us, either by embryology or by palaeontology.

Special praise is due to the illustrations, which have nearly all been drawn for the work, under Prof. Roule's direction, by Mr L. Jammes, in a style that is at once original and effective. We must, however, protest against the picture on p. 1275, purporting to represent living crinoids, "dans un fond rocheux de convention" (penny peep-show convention). Attached to one of these marvellous rocks, by a stem far too short in proportion to its arms, is a *Pentacrinus*; how it is fixed one cannot tell, but certainly not by the cirri, as

undoubtedly it would be in life. Below it, on a bottom that is apparently muddy, is a *Holopus*, which certainly ought to have been attached to the rocks, though not on the same page as the *Pentacrinus*, of which genus there appear to be other specimens, wildly waving about on a vertical precipice in the background. Fortunately, this is the only picture of the kind. The printing of the book is excellent, but the type and paper used have made the volumes rather too portly for comfort. With the works of the two Perriers, of Delage and Hérouard, of Blanchard with his corps of specialists, and of Roule, our friends across the Channel suffer from no lack of home-made text-books. And on this they are distinctly to be congratulated.

Plant Life

Plant Life Considered with Special Reference to Form and Function. By Charles Reid Barnes, Professor of Plant Physiology in Chicago University. 12mo, pp. x+428, with 415 text-figures. Holt: New York, 1898. Price $1.12.

We have nothing but praise for this excellent introduction to the study of plants. The author describes it as an attempt to exhibit the variety and progressive complexity of the vegetative body; to discuss the more important functions; to explain the unity of plan in both the structure and action of the reproductive organs; and finally to give an outline of the more striking ways in which plants adapt themselves to the world about them. It is meant to supplement genuine and regular work in the laboratory. There are four parts: Part I. The vegetative body, traces the increase in morphological differentiation from the unicellular organism to the seed-plant, and then discusses the general structure of root, shoot, stem, and leaves. As regards the terms primary and secondary, we note that the former is used to express the original root developed from the egg, the latter being applied to adventitious roots wherever developed. Part II. Physiology, deals with the general facts of the physiology of the individual, namely, maintenance of form, nutrition, growth, and movement. Reproduction is treated separately, under the headings vegetative and sexual, in Part III. Part IV. Ecology, contains chapters dealing with forms of vegetation in relation to different sets of conditions; Mesophytes, or the ordinary land plants with which dwellers in fertile temperate climates are acquainted; Xerophytes, or plants adapted to dry conditions; Hydrophytes, or those adapted to a more or less aquatic life. This part also comprises chapters on symbiosis, the relations of plants to animals, and the protection and distribution of spores and seeds. There are several useful appendices, including directions for a course of laboratory work, and for collecting and preserving material, with lists of apparatus, reagents, and reference books. An important feature of the volume is the great number of excellent figures, with an unusually full explanation in each case.

The Physiological Study of Plants

Practical Plant Physiology: an Introduction to original research for students and teachers of Science, Medicine, Agriculture, and Forestry. By Dr W. Detmer. Translated from the second German edition by S. A. Moor. 8vo, pp. xx+556, with 184 illustrations. London: Sonnenschein & Co. 1898. Price 12s.

Yet another botanical hand-book translated from the German! The translator's desire in undertaking this work is to promote the teaching

of plant physiology in England, where he thinks it is seriously retarded by the lack of suitable books. He admits the excellence of Francis Darwin's "Practical Physiology of Plants," but seems to think that an English work on more advanced and comprehensive lines is needed.

Prof. Detmer's "Praktikum" covers nearly the whole field of experimental plant physiology in the widest sense, ranging from the rheotropism of Myxomycetes to the breaking stress of bast fibres, and from unpalatability with snails to emulsion figures simulating protoplasm. Yet the whole does not strike one as being, in the highest sense, a book—an expression of a personality—as does the "Experimental Physiologie" of Sachs, or even the "Praktikum" of Prof. Strasburger; but it has rather markedly the air of being pieced together. It is indeed an encyclopaedia of methods, which have been carefully overhauled—an immense piece of work—by the author. It seems as if it ought to be very valuable as a work of reference, yet one is not quite sure what class of student will refer to it. It is no doubt really intended for the small number of advanced students who are about to undertake research on the physiology of plants, and so gives an account of the stock methods of investigation.

The experiments are grouped, in logical sequence, into five sections, viz., the Food of Plants, the Molecular Forces in Plants, the Metabolic Processes in the Plant, Movements of Growth, and Movements of Irritation. The successive experiments are not categorically limited, but are linked together by theoretical and expository paragraphs, so that the book can be read continuously; but this involves so much additional space that in many experiments small details have to be omitted, the neglect of which will prevent the experiment being successful on first trial.

The second German edition (1895) is nearly one-third longer than the first edition (1888), has been largely re-written, and contains a short appendix on recent views on the ascent of water.

The weakest section in the present edition is that on the application of the polariscope. It is said to be 'very instructive' to investigate the phenomena exhibited in polarised light by starch grains (which, however, should be mounted in Canada balsam for this purpose, not in water); but surely, beyond the pretty effects, the student learns nothing from his observation but the fact that starch grains are anisotropic. This may be due to a remote crystalline structure or to internal tensions. The latter hypothesis is not mentioned though it might easily have been illustrated experimentally. As a further obscurity, 'gypsplättchen' has been translated in the English edition as 'plates of gypsum' (p. 115). This should of course be plates of selenite, which have special optical properties.

The most complete sections are those on Respiration, which have been considerably expanded, and to which Prof. Detmer and his pupils have contributed original work.

The English edition is translated without alteration or addition from the second German one. There is something to be said for retaining unchanged the appendix, which gives an annotated list of the German dealers in, or makers of, the scientific apparatus described in the book; but a patriotic editor might have indicated where some of the articles could be obtained in this country. It is not necessary

to send to Germany for klinostats and polar planimeters any more than for platinum crucibles and india-rubber tubing.

The translator has done his work excellently, and we are but rarely reminded of the German original. We note, however, a few errors. The English of 'plasmolytische gemacht' is 'plasmolysed,' not 'rendered plasmolytic' (p. 145). On p. 43, l. 19, 'layer' would be intelligible while 'meniscus' is not, though the German is 'Meniscus.' On p. 424, l. 16, 'favourable' is found where 'unfavourable' is meant.

The abundant matter of this compendious book is conveniently arranged, and the translation of it will be welcomed by the proportionally increasing number of those among even serious botanical students, who are unable to use such a work in the original.

The Organism as Unit

Problems of Biology. By George Sandeman. 8vo, pp. viii+213. London: Swan Sonnenschein. 1896.

The Living Organism: An Introduction to the Problems of Biology. By Alfred Earl. 8vo, pp. xiv+272. London: Macmillan & Co. 1898. Price 6s.

These two books appear to have similar aims and to arrive at much the same ultimate conclusion, or absence of conclusion, and yet they stand in strong contrast to each other. Mr Earl's book is nicely printed and bound; it is written in an easy style, has all the clearness that can be imparted by division into chapters with many subdivisions and running headlines, uses italics where appropriate, and is furnished with a full table of contents and with an index. Mr Sandeman's book is needlessly repellent in type and binding; its style has an individuality that is strongly marked, but far from attractive or lucid; the chapters and the paragraphs are of wearying length; there is no analysis of contents, and no index. Nevertheless, we set Mr Sandeman's book above Mr Earl's: it is more interesting, more critical, and more suggestive. If the author would rewrite it with more feeling for the dulness of his readers, if he would be guided by the example of Mr Earl, and if he would temper his biological erudition to the ignorance of the philosopher, his philosophical jargon to the simplicity of the biologist, we should recommend both parties to read his second edition. We dare not recommend the perusal of the first, except to those superior beings who have mastered "Sordello" and "Bygmester Solness."

For both of these writers a theory of the unity of the organism forms the chief object of biological enquiry. Each of them is at pains to tell us in what this unity does not consist; but, as is natural, neither of them can formulate a clear conception of what it is. Mr Earl lays stress on the impossibility of conceiving the organism apart from its environment; they may be expressed in terms of subject and object, and constitute "a dual manifestation of a single reality." Mr Sandeman dallies with 'feeling' as that which gives unity to the organism, but presently rejects it and falls back on the barren conception of 'character,' which, he says, "is the identity in difference

of concrete individuals, and is the familiar expression for the whole of a system," that is to say, a pure abstraction. To us the most fruitful conception appears to be that of 'memory'; in other words, the tendency to repeat the same action or process under similar, or almost similar, conditions. What, if any, may be the ultimate physical cause that causes a given readjustment of molecules in any one mass of protoplasm to be repeated rather than replaced by another adjustment, we do not know. But if this tendency, not unknown in the inorganic world, be admitted for protoplasm, then at least we have a phenomenal foundation for theories of specific segregation, individuality, and heredity.

Mr Sandeman's critical artillery is levelled at the three postulates of biology:— (1) "that the qualities of the individual are separate constituent elements of which the organism is the total sum"; (2) that "all the qualities of the organism and all its stages are the manifestation of, and are related to, one another only through an agent or system of agents within the known body"; (3) that "everything organic exists only by reason of, and is to be explained only in relation to, some special external use which it now has, or which a similar structure has had in former times." The demolition, from a philosophical standpoint, of various biological theories is well worth reading by practical naturalists. Mr Earl's book should also be read by them, for it is to be feared that in our modern schools of science hardly enough attention is paid to the logic and fundamental conceptions of the subject. "The dissection of typical organisms is not necessarily an intellectual exercise." F. A. B.

Diet and Blood

Natural Hygiene; or, Healthy Blood the essential condition of Good Health, and how to attain it. By H. Lahmann, M.D. Translated by Dr H. Büttner. 8vo, pp. viii + 254. 5 plates. London: Swan Sonnenschein & Co. 1898. Price 4s. 6d.

Dr Lahmann is a vegetarian and an enthusiast; his book has passed through many editions in Germany, and has been translated into other tongues; now for the first time it appears in English. The author is able to show a considerable amount of scientific reason for the faith that is in him, which may be epitomised in three propositions:—(1) that ordinary diet is deficient quantitatively and qualitatively in the mineral salts required by the human body; (2) that we consume far too much sodium chloride; and (3) that we take in water to excess. The truth of at least the first two of these propositions is incontrovertible, and Dr Lahmann deserves credit for calling attention to them. On this basis he builds his doctrine of "dietetic dysaemia," and proceeds to expound a new pathology for most of the ills that flesh is heir to—from short sight to difficult labour. In our opinion, he falls into the error of pushing his doctrine to unwarrantable extremes—the fate of most enthusiasts. No intelligent person can fail to admit the force of certain of his contentions, and no sane pathologist can repress a smile at others. The book is worth reading, and is likely to do much more good than harm.

Introductions to Chemistry

Chemistry for Schools: an Introduction to the Practical Study of Chemistry. By C. Haughton Gill. Tenth Edition. Revised and enlarged by D. Hamilton Jackson. Crown 8vo, pp. x+356, with 105 figs. London: Stanford. 1898. Price 4s. 6d.

The Organized Science Series. First Stage. Inorganic Chemistry (Practical). By F. Beddow. 8vo, pp. viii+166, 37 figs. London: Clive. 1898. Price 1s.

Of the making of elementary text-books of chemistry there appears to be no end. We have recently received copies of the above works, and although it scarcely enters into our province to review them critically, we may say that they both appear to be very clearly written, and to be well adapted to the requirements of the student. Neither of them differs very startlingly from others of the kind. Perhaps of the two the smaller book shows more originality of treatment. We are glad to see that, even in so elementary a book, at least thirty pages are devoted to quantitative experiments. In "Chemistry for Schools," the five-page chapter on Crystalline Systems is quite inadequate, and in parts unintelligible. It is time that text-books of chemistry contained really clear and detailed expositions of at least the elementary principles of crystallography.

L'Année Biologique.

We welcome the second volume of this excellent 'Biological Record,' even though it be issued some twenty months after the last of the publications with which it professes to deal. In regard to accuracy of quotation and comprehensiveness, it is an improvement on the first volume, noticed in *Natural Science* for August 1897. As we said before, absolute completeness is hardly to be hoped for, and certainly is not attained by Professor Delage and his collaborators. For instance, although most of the appropriate papers that appeared in our own pages during 1896 are indexed, we see no reference to Miss Newbigin's valuable contribution on the pigments of animals; or does this not come under 'Biology'? However, there is a list (in itself useful) of nearly 900 periodicals said to have been consulted in the preparation of the volume. The abstracts, so far as we have checked them, seem done with intelligence and accuracy; critical remarks are, as a rule, inserted between square brackets. At the beginning of each subject an attempt is made to give a general view of advance in that field, and in certain cases this has led to the publication of elaborate essays. Such are that on phagocytosis in the animal kingdom, by J. Cantacuzène, with preface by E. Metchnikoff, and that on marine zoogeography, by G. Pruvot. The publishers are Schleicher Frères, 15 Rue des Saints-Pères, Paris, and the price is 20 francs.

Varia

We have received from Messrs Friedländer & Son, of Berlin, a copy of *Naturae Novitates* for 1897. This valuable record is issued in parts twice a month; and finally indexed, bound, and sold for four marks, at the middle of the succeeding year. It is a record of all books that appear dealing with Natural History and the exact Sciences, and is invaluable in its fortnightly form for ready reference, as well as in

its annual form for general reference. *Naturae Novitates* provides moreover a regular and important collection of Personalia as regards appointments and deaths.

THE second part of vol. i. of the *Records of the Botanical Survey of India* contains a paper on the results of Lieut. E. Pottinger's journey through Myitkyina, Burmah. There is a complete list of the phanerogams and vascular cryptograms and a map. Many orchids are recorded.

THE re-issue of Stanford's "Compendium of Geography" will be completed in 1899. Vol. i., by Geo. C. Chisholm, is complete, and contains the birth of the mainland of Europe. Vol. ii. will include the British Isles, Scandinavia, Denmark, and the Low Countries, and is already advanced. Other volumes, to contain Central and South America, are in the hands of Sir Clements Markham and A. H. Keane.

THE *Annual Progress Report* of the Geological Survey of Western Australia for 1897 has just reached us, in the contorted and unmanageable condition usual with these official publications. The report consists of 66 pages and no less than seven valuable maps (Northampton, Peak Hills, Horseshoe Diggings, Bunbury, Kanowna, Coolgardie, and Artesian Bores in the vicinity of Perth), all of which have accompanying text.

OUR interesting contemporary *The Journal of School Geography* includes among the varied contents of its October number an illustrated article by G. K. Gilbert on the "Origin of the Physical Features of the United States," reprinted from the *National Geographic Magazine* for July. There is also a useful note by A. J. Herbertson, defining the various terms Britain, Great Britain, England, &c.

FURTHER LITERATURE RECEIVED.

Aids in Practical Geology, Cole: Griffin, London. Text-book of Botany, Vines: Sonnenschein, London. Text-book of Mineralogy, Dana: Wiley, New York. Cytologie expérimentale, Labbé: Carre & Naud, Paris. Allgemeine Biologie, I., Protoplasma, Kassowitz: Perles, Wien. Pflanzen-geographie, Schimper: Fischer, Jena. From Matter to Man, Dewar: Chapman & Hall, London.

Studies from Yale Psychol. Lab., vol. v. Report Australian Museum, 1897. Cornwall County Council, Report Fisheries. Study of the development of the Tuatara, Denby: *Proc. Roy. Soc.* Relations between marine animal and vegetable life, Vernon: *Mitt. Zool. Stat. Neapel.* Birds from Sierra Nevada de Sta Marta, Bangs: *Proc. Biol. Soc., Washington.* Ruins of Xkichmook, Thompson: *Field Columbian Mus. Publ.*, 28. Half-century of Evolution, Packard: *Proc. Amer. Assoc.* Report on Chillagoe mining district, Jack: Geol. Surv., Queensland. Relations between Hybrid and Parent forms of Echinoid. Larvae, Vernon: *Phil. Trans.*

Amer. Geol., Jan., Feb., March, Nov.; Amer. Journ. Sci., Oct.; Amer. month. Micr. Journ., Sept., Oct.; Amer. Nat., Oct.; Bolletino del Naturalista, 10; Botan. Gazette, Oct.; Feuille des jeunes Nat., Nov.; Irish Nat., Nov., Oct.; Scot., Geogr. Mag., Nov.; Timehri, XII., 1; Trans. Perthshire Soc., II., 6; Victorian Nat., Sept. and Oct.; Westminster Rev., Nov.; Journ. Conchol., Oct. (vol. ix., No. 4; Journ. School. Geogr., Oct., Nov.; Knowledge, Nov.; Literary Digest, Oct., 8, 15, 22, 29, Nov. 5; Naturae Novit, Oct., No. 18, 19, 20; Naturaleza (Mexico) (2) II., III.; Naturalist, Nov.; Nature, Oct. 20, 27, Nov. 3, 10, 17; Nature Notes, Nov.; Naturen, Oct.-Nov.; Proc. R. Soc. Victoria, XI., 1; Plant World, Oct.; Psychol. Rev., Nov.; Review of Reviews, Oct., Nov.; Revue Scient., Oct. 22, 29, Nov. 5, 12, 19; Riv. Ital. Sc. Nat., Nov.-Dec.; Science, Oct. 7, 21, 28, Nov. 4, 11; Scientific Amer., Oct. 8, 15, 22, 29, Nov. 5, 12; Scot. Med. and Surg. Journ., Nov.

OBITUARIES

JAMES SPENCER, the geologist and palaeobotanist, died on 9th July at Akroydon, Yorkshire. He was born 27th April 1834 at Luddenden, and for a time worked in a brickyard. He was "discovered" by Colonel Akroyd about 1853, and by him given a post as porter in his warehouse, from which position he speedily rose to that of cashier, remaining in Colonel Akroyd's firm until he retired in 1886. He was connected with the Haley Hill Literary and Scientific Society, the Halifax Scientific Society, and the Ovenden Naturalists' Society in many ways, and did a great deal to spread a knowledge of geology among his fellows. Spencer became actively associated with the late Professor Williamson about 1878, and was an important helper in the "Fossil Plants of the Coal Measures." A full account of his life and work appears, by Mr W. B. Crump, in the *Halifax Naturalist* for October.

DR JOHN EDWARD TIERNEY AITCHISON, the well-known botanist and explorer, died on September 30, at Kew, aged sixty-three years. He was born in India in 1835, took his degrees in medicine and surgery at Edinburgh and entered the Bengal medical service in 1858, retiring in 1888. He paid especial attention to the botany of India, publishing his first paper in 1863 on the plants of the Jhelum district, with notes of considerable economic value. In 1869 he issued a catalogue of the flora of the Punjab and Sindh, and in 1878 he accompanied Roberts' expedition to the Kuram, acting as botanist. He made immense collections, which were worked out at Kew, the results appearing in the Linnean Society's publication. He obtained not less than 15,000 specimens from the Thal and Peiwarkotal districts in one expedition, and 10,000 on another occasion when he acted as naturalist to the Afghan Delimitation Commission. His labours led him over almost the whole of North-west India, Afghanistan, Baluchistan, Persia, and Russian Turkestan, and he was a botanist who had a keen idea of other things than mere dried plants. He has left much material which we hope will be worked out. We are indebted for these notes to a sympathetic article, by Mr Botting Hemsley, in *Nature*.

LUIGI LOMBARDINI, Professor of the Anatomy of Domestic Vertebrates at the Veterinary School of Pisa, died at that place on 27th June, having been born at Poggibonsi on 11th April 1831.

The deaths have also been announced of:—W. G. ATHERSTONE, who worked and wrote on the geology of South Africa between 1856 and 1874; on 1st September, at Hobart, Tasmania, the well-known Australian conchologist, C. E. BEDDOME; on 5th August, Prof. EUGENIO BETTONI, director of the fishery station at Brescia, aged 53; J. CROCQ, professor of pathology in the University of Brussels; GIUSEPPE GIBELLI, professor of botany and director of the Botanical Garden at Turin, on 16th September; on 25th September, Dr A. LASARD, at Nizza, aged 74; Don FRANCISCO COELLO DE PORTUGAL, president of the Geographical Society of Madrid; HEINRICH THEODOR RICHTER, lately director of the School of Mines at Freiburg; MICHELE STEFANO DE ROSSI, the seismologist; on 31st May, at Wilmington, Delaware, U.S.A., the botanist, EDWARD TATNALL, aged 80.

NEWS

The following appointments have recently been made :—Miss Catherine A. Raisin, D.Sc., the well-known petrologist, to be Vice-Principal of Bedford College for Women, London ; C. B. Crampton, M.B. of the University of Edinburgh, to be assistant-keeper in the geological department of the Manchester Museum, in succession to H. Bolton ; Prof. C. Chun to be professor of zoology at Leipzig ; Prof. W. Kükenthal to be professor of zoology at Breslau ; Dr Conrad Keller of the Polytechnicum, Zürich, to be full professor of zoology ; Dr H. E. Ziegler, of Freiburg, i/B, to be professor of phylogeny at Jena ; Dr C. A. Kofoid to be assistant-professor of zoology at the University of Illinois ; Wallace Craig to be assistant in the State Laboratory of Natural History at the Illinois Biological Station ; E. B. Forbes to be field-entomologist of the Illinois State Laboratory of Natural History ; J. H. M'Gregor to be assistant in zoology at Columbia University, New York ; G. M. Holman as assistant in biology at the Massachusetts Institute of Technology ; Dr B. Moore, of University College Hospital, to be professor of physiology in the Yale Medical School ; Dr Albert Matthews to be assistant-professor of physiology at Tufts College ; Dr Simon Flexner to be professor of pathological anatomy at Johns Hopkins University ; Dr Joseph Priestly to be teacher of hygiene in the British Institute of Preventive Medicine ; Dr J. P. Hylan to be assistant professor of psychology at the University of Illinois ; Albert Gaillard to be director of the Lloyd Herbarium at Angers ; C. W. Young to be assistant in botany at the University of Illinois ; James Pollock, Hamilton Timberlake, and Julia W. Snow to be instructors in botany at the University of Michigan ; F. O. Grover, of Harvard, to be instructor in botany at Oberlin College, Granville. Ohio.

Dr Fredrik Wilhelm Christian Areschoug has resigned the professorship of botany at Lund University.

Prof. E. B. Wilson, of Columbia University, has recently recovered from a serious illness, and will spend next year in travel and research abroad.

Prof. Flinders Petrie has presented to the Museum of Anatomy and Anthropology at Cambridge, nineteen cases of skulls and bones from his excavations at Hieraconopolis. These include remains of the prehistoric and earliest dynastic races in Egypt.

Lord Walsingham, High Steward of the University of Cambridge, has offered a second medal, in bronze, for specially meritorious essays in biology which do not obtain the Walsingham gold medal.

The University of Sydney is to be affiliated to that of Cambridge, and students in arts or science who have pursued a certain course at Sydney will be admitted to the usual privileges of affiliated students.

During the absence of W. H. R. Rivers with Prof. Haddon's Expedition, the course in experimental psychology at University College, London, is being directed by Mr E. T. Dickson.

Prof. J. W. Traill is to be director of the Cruickshank Botanical Garden at Aberdeen University.

Mr Briggs S. Cunningham, of Cincinnati, has given $60,000 towards the erection of a building for biology and physics at Cincinnati University.

We learn from *Science* that the U.S. Fish Commissioner has presented Cornell University with a collection of fresh-water and salt-water fishes, numbering between four and five hundred thousand specimens. The collection, in so far as

it consists of living fishes, will be of great value not only to the zoological department, but also to the College of Forestry, in which a course in pisciculture and venery is to be introduced. Duplicates of this collection are to be presented to other institutions.

Provost Harrison, of the University of Pennsylvania, has been elected president of the Wistar Institute of Anatomy, in succession to the late Dr William Pepper.

The British Institute of Preventive Medicine has recently assigned a large laboratory at Chelsea to research and teaching in technical bacteriology ; it will be named the Hansen Laboratory and be under the direction of Dr G. Harris Morris. The formal opening of the Institute will take place early next year.

According to *Science*, the University of Pennsylvania and the Academy of Natural Sciences have received from Alaska nearly 13,000 specimens, secured near Point Barrow by an expedition under the management of E. A. M'Ilhenny of Louisiana, fitted out and conducted by N. G. Buxton of Ohio and W. E. Snyder of Wisconsin. The zoological and botanical specimens go to the Academy, the ethnological to the University.

Mr Alan Owston of Yokohama has recently sent to this country a magnificent collection of hexactinellid sponges from the seas of Japan. Most of these have been purchased by the Trustees of the British Museum, but a fair number have gone to Oxford. Among the specimens are many studied by Professor Ijima for the monograph that he is writing on the group.

Mr Michael Lakin's donation of a large Liassic *Ichthyosaurus* to the British Museum, already announced in these pages, has necessitated a considerable rearrangement of the existing collection. We understand that the old cases are to be removed, while the fine slabs containing these fossils will be simply covered with glass and exhibited upon the wall. Space is to be gained by raising a number of the specimens above the top of the present wall-cases.

Dr Jonathan Hutchinson, whose educational museum at Haslemere is well known, is starting a similar establishment at his native town of Selby in Yorkshire. The building has already made considerable progress.

On October 5, a new natural history museum was opened at King Williams Town, Cape Colony.

The U.S. Department of Agriculture has sent Mr M. A. Carleton to Russia to study cereals.

The New York Botanical Gardens makes rapid progress. The museum building is complete up to the second storey, the skeleton of the whole being in place. The planting of the border will, says *Science*, be completed during the autumn. This will be about two miles long, and will contain some three hundred and fifty varieties of trees and shrubs.

A fifth International Congress of Hydrology, Chinatology, and Geology was held at Lüttich from September 25 to October 3.

The Sixth International Otological Congress will be held in London at the Hall of the Royal Colleges of Physicians and Surgeons, from August 8th to 12th of next year. The last meeting of the Congress was held three years ago at Florence, under the presidency of Professor Grazzi.

The International Conference on the Bibliography of Scientific Literature met at Burlington House, London, in the rooms of the Society of Antiquaries on October 11-13. It was attended by the following delegates :—Austria, Professors L. Boltzmann and E. Weiss ; Belgium, Chevalier Descamps and Messrs P. Otlet and H. La Fontaine ; France, Prof. G. Darboux, Dr J. Deniker, and Mr E. Mascart ; Germany, Prof. Klein of Göttingen ; Hungary, Drs A. Heller and

T. Duka ; Japan, Prof. E. Yamaguchi ; Mexico, Señor Don Francisco del Paso y Troncoso ; Netherlands, Prof. D. J. Korteweg ; Norway, Dr J. Brunchorst ; Sweden, Dr E. W. Dahlgren ; Switzerland, Drs J. H. Graf and J. Bernoulli ; United Kingdom, Sir John Gorst, Professors M. Foster, A. W. Rücker, and H. Armstrong, Sir Norman Lockyer and Dr L. Mond ; United States, Dr C. Adler ; Cape Colony, Mr R. Trimen ; India, Lieut.-General Sir R. Strachey and Dr W. T. Blanford ; Natal, Sir Walter Peace ; New Zealand, Hon. W. P. Reeves ; Queensland, Hon. Sir Horace Tozer. The chief decisions were : that the catalogue be published in the double form of cards and books ; that geography be limited to mathematical and physical, excluding political and general geography ; that a separate schedule be provided for each of the following branches of science,—Mathematics, Astronomy, Meteorology, Physics, Crystallography, Chemistry, Mineralogy, Geology (including Petrology), Geography, Palaeontology, Anatomy, Zoology, Botany, Physiology, (including Pharmacology and Experimental Pathology), Bacteriology, Psychology, Anthropology ; that Italian should be added to the list of languages not requiring translation, the others being English, French, German, and Latin. The following were appointed a Provisional International Committee : — Professors Armstrong, Descamps, Foster, Poincare, Rücker, Waldeyer, and Weiss, and Dr S. P. Langley ; the delegates were requested to organise local committees in their respective countries, to discuss matters connected with the catalogue, and to report within six months to the committee just mentioned ; and this committee is to frame a report not later than July 31, 1899. As regards future working, there is to be a Central Bureau with a Director, and as many Regional Bureaux as can be persuaded to act ; these are the workers. But there are to be International Conventions held in 1905, 1910, and every tenth year afterwards, to reconsider and, if necessary, revise the rules now drawn up. There is also to be an International Council, which shall meet in London once in three years at least ; this is to be the supreme authority over the Central Bureau. It is recommended that the International Council shall appoint for each science included in the catalogue five persons to form an International Committee of Referees, so far as possible representative of the constituent regions. It shall be the duty of the director of the Central Bureau to consult the appropriate committee or committees, by correspondence or otherwise, on all questions of classification not provided for by the catalogue regulations ; or, in cases of doubt, as to the meaning of those regulations. There are a good many more cumbrous rules, but those we have last quoted are about enough to wreck any scheme less difficult and less enormous than the present one. It is to be hoped that the director will not do his "duty," but go ahead.

THE Royal Society has awarded its medals as follows :—Copley Medal, Sir William Huggins ; Royal Medals, Rev. John Kerr and Mr Walter Gardiner ; Rumford Medal, Prof. Oliver Lodge ; Davy Medal, Prof. Johannes Wislicenus ; Darwin Medal, Prof. Karl Pearson.

GENERAL regret is felt that Sir John Evans should have found it necessary to resign the treasurership of the Royal Society. Regret has also been expressed that the Council should not have found a more eminent man of science than Mr A. B. Kempe, the mathematician, to succeed him.

THE Mineralogical Society, at its anniversary meeting on November 15, elected the following officers for the ensuing year :—Prof. A. H. Church, president ; Prof. G. D. Liveing and Dr Hugo Müller, vice-presidents.

THE late Mrs Stainton has bequeathed to the Entomological Society, London, such entomological works from her husband's library as were not already in its possession. Many of these are old and scarce.

THE Geologists' Association, London, opened its session on Friday, November 4th, with a remarkably successful conversazione in the Library at University College. The following were among the numerous exhibits :—Specimens illustrating the artificial production of the structure of gneissose rocks by the deformation of heterogeneous masses of clay, and photographs of similar structures in the gneissose rocks of the Lizard Peninsula ; by the president, J. J. H. Teall. A fine series of Russian and Indian ammonites ; by Prof. J. F. Blake. Specimens and micro-sections of rocks collected in Russia during the visit of the International Congress ; by the secretary of the Association, P. Emary. Illustrations of the modes of occurrence of magnetic iron ore at Gellivara and other localities in Lapland ; by D. A. Louis. Apatite and associated rocks from Canada ; by Rev. Prof. T. G. Bonney. Granites and gneisses from the Vosges and Switzerland ; by Miss C. A. Raisin. A beautiful set of photomicrographs of recent and fossil foraminifera ; by H. W. Burrows and R. Holland. Collections of pebbles illustrating the constitution and sources of origin of high-level gravels in S. and E. of England ; by A. E. Salter. Remains of mammoth, red deer and *Bos primigenius* dredged from the Dogger Bank, as well as other interesting specimens ; by W. F. Gwinnell. A remarkable series of palaeolithic implements from West Wickham, Kent ; by A. S. Kennard. Skiagrams of fossil starfish from the Devonian of Germany, said to show structures invisible on the surface of the stone, but the originals were not brought for comparison ; by Upfield Green. Specimens, drawings and plaster casts of *Petalocrinus*, the paddle-armed Silurian crinoid ; by F. A. Bather. A fine siluroid fish from late Tertiary oil-shales of Taubaté in the Province of São Paulo, Brazil ; by J. N. Tervet. Jaws of a new dinosaur found by J. David in the Rhaetic of Glamorganshire, which jaws, we are told, will speedily chaw up certain guesses of an eminent palaeoherpetologist ; by E. T. Newton. Sir Archibald Geikie exhibited some of the new survey maps, in which many improvements have been made possible by the improved topography of the new ordnance maps ; also beautiful collotype plates which will illustrate the forthcoming memoir on the Silurian rocks of Scotland, by Peach and Horne, with petrographical notes by Teall. Many other exhibits and an enthusiastic crowd bore witness to the energy of this useful Association, and of its secretary, Mr Emary, whose address is 12 Alwyne Square, Canonbury, London, N.

THE Society for the Protection of Birds announces that it has now a number of beautiful lantern-slides ready to lend to any of its members who may be able to arrange for an illustrated lecture on birds and their protection. Application for these should be made direct to the Secretary, 326 High Holborn, London, W.C.

THE Report and Transactions of the South Eastern Union of Scientific Societies containing the proceedings at the Third Annual Congress held at Croydon, June 2nd, 3rd and 4th 1898, is to hand. It contains :—Places of meeting and presidents, officers for 1898-9, rules, list of affiliated societies with names of delegates, secretaries, &c., report of delegates' meeting, secretary's report, balance sheet, photographic secretary's report, lantern-slide scheme, postal magazine club, reception and exhibits, botanical research committee, referees, annual address, and the various papers read before the Congress. We have already given the titles of these, and have published two of them ; the paper on Dene-holes, by C. Dawson, has also been published in the *Geological Magazine*. This should surely have been mentioned in the report. Here, however, Mr Dawson's paper on Natural Gas is embellished by two illustrations of the Heathfield flame, one of which appeared in *Black and White*. The photographic secretary reports that a set of seventy-eight lantern-slides illustrating the Greensand and Gault has been got together, and may be borrowed by members, along with a written lecture on the subject by H. E. Turner. It is proposed to form new sets dealing with (i) Prehistoric man in S.E. England ; (ii) English wildflowers, with special referen ce

to forms of capsules and their dehiscences ; (iii) Coast erosion in S.E. England ; (iv) Marine organisms as transparent lantern-slides. Those willing to help this excellent project should communicate with Mr H. E. Turner, 2 Bouverie Road West, Folkestone. Another proposal is the formation of a postal magazine club, whereby members will doubtless be made acquainted with at least the titles of many magazines, but which will probably tend to lower the circulation of some publications yet further. It is an example we scarcely wish to see followed. The next Congress will be held in Rochester early in June 1899 under the presidency of W. Whitaker.

THE Hull Scientific and Field Naturalists' Club opened its winter session on October 12, with a paper by F. W. Fierke, its treasurer, on "The ancient meres of Holderness and their Contents." Subsequent papers have been by J. Hollingworth on "Public Health," by Rev. H. P. Slade on "Our Water Supply." Among forthcoming items we note J. R. Boyle on "The organisation of an English Manor," and J. J. Marshall on "The Mosses of the East Riding." It is satisfactory to see how many papers are careful studies of local subjects rather than the generalities so dear to many societies of the kind. The president is R. H. Philip ; the secretary, T. Sheppard, 78 Sherburn Street, Hull.

RECENT papers read before the Oxford University Junior Scientific Club have been by Dr Gustav Mann "On the Origin of Life," and by Mr A. D. Darbishire "On Natural Selection among Lepidoptera." The *Transactions* of the Club are now issued separately from the notices of meetings, and will be sent to life members post free for one year for three shillings. No. 5, recently received, contains a "Preliminary note on changes in the gland cells of *Drosera* produced by various food materials," by Lily H. Huie, and a paper on "Turpentining [*i.e.* obtaining turpentine from trees] in the Southern States [of America]," by H. E. Stapleton.

THE Sheffield Literary and Philosophical Society have presented Dr H. C. Sorby with his portrait, by Mrs M. L. Waller, in celebration of his fifty years' connection with the society (1847-1897).

THE British Mycological Society held its second annual meeting in Dublin, September 19-24. Excursions were made to Howth, Powerscourt, Brackenstown near Swords, the woods of Avoca, Lucan, and Dunran. These resulted in an addition of sixteen species to the fungus flora of Dublin and Wicklow ; a list will appear in the December number of the *Irish Naturalist*. Dr Plowright, president for the current year, delivered an address, discussing certain fungi figured in Cooke's "Illustrations." Papers were read by Messrs Wager, Crossland, M'Weeney, Soppitt, and Rea.

ON Oct. 18, Dr John William Toore was elected president, and Dr W. J. Smyly vice-president, of the Royal College of Physicians in Ireland for the ensuing year.

THE Naturalists' Society of St Petersburg has erected a new biological station on Lake Bologoy ; it is specially intended for the study of plant-plankton.

THANKS to agitation begun by the Field Naturalists' Club of Victoria, Wilson's Promontory in that Colony has been proclaimed a national park for the preservation of native fauna. This Club is also doing its best to suppress the ruthless destruction of the wattles (acacias) for the sake of their flowers. The subject has also been taken up by the Australian Natives Association.

THE Scientific Alliance of New York consists of the Torrey Botanical Club, the New York Microscopical Society, the New York Section of the American Chemical Society, the New York Mineralogical Club, the American Mathematical Society, the Linnean Society of New York, and the New York Entomologi-

cal Society. It is to be hoped that some day the Alliance will have a building of its own, for at present these societies have to meet at various buildings in the city.

THE fourth annual meeting of the Botanical Society of America, held at Boston, under the presidency of Dr N. L. Britton, on August 19 and 20, was most successful. Prof. L. M. Underwood was elected president for 1899. The address of Prof. J. M. Coulter, the retiring president, was on "The origin of Gymnosperms and the Seed Habit," and has already appeared in *Science*. The other papers of interest to our readers were "On Sporogenesis in *Arisaema*," by Prof. G. F. Atkinson; "Symbiotic Saprophytism," by Prof. D. T. Macdougal; "Sporogenesis in *Trillium*," by Prof. G. F. Atkinson; "Structure and development of the Centrosphere in *Corallina*," by Dr B. M. Davis; "Relations between the Forest Flora and Geological Formations in New Jersey," by Dr Arthur Hollick; "Notes on the Fertilisation of the white Pine," by Miss M. C. Fergusson; "A *Helianthus* from Long Island," by Dr N. L. Britton; "Tetradformation in Tsuga," by W. A. Murrill; and "A fossil moss from the State of Washington," by Mrs Britton and Dr Hollick.

THE American Forestry Association, we learn from *Science*, held a meeting from August 23 to 25, at Boston. Reports were presented by various States as to the condition of the forestry movement. The most important feature of the meeting was the discussion of the aims and objects of the newly-established State College of Forestry at Cornell, by its director, Dr Fernow. *Science* promises to print his address in full.

THE Commission appointed by the Colonial Office and the Royal Society to investigate the mode of dissemination of malaria with a view to devising means of preventing the terrible mortality which now takes place among Europeans resident in tropical and sub-tropical climates will consist of Dr C. W. Daniels, of the Colonial Medical Service, British Guiana; Dr J. W. W. Stephens, formerly Lawrence Student in Pathology and Bacteriology at St Bartholomew's Hospital; and Dr R. S. Christophers, of University College, Liverpool. Dr Daniels will proceed at first to Calcutta, where he will acquaint himself practically with the remarkable work which Surgeon-Major Ross, of the Indian Medical Service, is carrying on into the relation of mosquitos to the dissemination of malaria. Drs Stephens and Christophers will spend some time in Rome studying malaria. Subsequently the Commissioners will meet at Blantyre, British Central Africa.

A COMMISSION—consisting of Dr Thomas R. Fraser, F.R.S., Professor of Materia Medica and Clinical Medicine at Edinburgh University (president); Dr Wright, Professor of Pathology at the Army Medical School, Netley; Dr Rüffer, who has been for some time head of the Egyptian Sanitary Department at Cairo; and two officers of the Indian Civil Service, Mr J. P. Hewett, C.I.E., and Mr A. Cumine, both of whom have had much to do with recent plague affairs in India—has been appointed to report on the following matters concerning the plague in India: (1) the origin of the different outbreaks; (2) the manner in which the disease is communicated; (3) the effects of certain prophylactic and curative serums that have been tried or recommended. The Commission reached Bombay towards the end of November.

MR E. W. L. HOLT has been appointed by the Royal Dublin Society to make researches over a period of five years on the life-history of the mackerel. A ship is being fitted up as a floating laboratory at Berehaven which will be Mr Holt's headquarters.

HAMBURG has founded at Freihafen a station for the protection of plants against injurious insects introduced from abroad, and against plant-disease generally.

THE half-yearly report of the inspector for the eastern sea fisheries district just issued, states that the fishing during this season has been much above the average. The rigid enforcement of the regulation against taking undersized shell fish has benefited the fishing-grounds. The chief offenders in this respect are the lobster and whelk fishers, whose practice it has been to use undersized crabs for bait. At one place alone the destruction of undersized edible crabs for this purpose has been upwards of 3,000 in each day's fishing, and the number of 'unsizeable' crabs destroyed along the Norfolk coast has been greater than the whole number of 'sizeable' crabs landed. The wholesome effect of prosecutions and convictions, and the increased efficiency of inspection during recent years, is shown by the fact that "all the fishermen report that the ground contains more small crabs than have ever been seen by them before."

CANADA is to have a floating biological station in the Gulf of St Lawrence for at least five years, and the Government of the Dominion has appropriated £1400 for the purpose. The board of management consists of Prof. E. E. Prince, director; Professors D. P. Penhallow and E. W. MacBride, of McGill University; Prof. Ramsay Wright, of Toronto University; Prof. L. W. Bailey, of the University of New Brunswick; Prof. A. P. Knight, of Queen's University; and Rev. V. A. Huart, of Laval University. For the first year the laboratory will be on the south shore of Prince Edward Island, and will be moved annually. It is hoped that active work may begin early in 1899.

MR GEORGE MURRAY, Keeper of the Botanical Department of the British Museum, has organised an expedition for the study of intermediate ocean depths. With the aid of the Royal Geographical Society, the Drapers' Company, and the Fishmongers' Company, he chartered the ss. "Oceana," which was fitted with deep-sea gear by the Silvertown Telegraph Cable Company. Mr Murray is accompanied by two of his colleagues, J. W. Gregory and V. H. Blackman, as well as by Dr Sambon, J. E. S. Moore, and Percy Highley, the last-mentioned acting as artist. The steamer, after some delay from fogs, left the Thames on November 16, and proceeded directly to the west coast of Ireland, where work was begun at the edge of the 100-fathom platform, about thirty miles west of Dingle Bay. It was intended to steam slowly for about 10 degrees westward, making continuous observations with a vertical chain of tow-nets, which would gradually be lowered until, with a length of 2000 fathoms, the series would include 38 nets. Thus the difference between the faunas of different depths in the same part of the ocean can be estimated by comparing the contents of the nets. Experiments with various forms of self-closing nets will be made for the sake of comparison. Soundings and observations of temperatures will be taken, and there may be opportunity for some deep-sea trawling.

A DETAILED natural history survey of the long sand-bank known as the North Bull, in Dublin Bay, recently undertaken by Messrs Praeger and Halbert, is, says the *Irish Naturalist*, turning out unexpectedly interesting from a zoological point of view, as this apparently inhospitable spot has already yielded several additions to the Irish fauna.

MR JOHN S. BUDGETT has started for the Gambia, under the instructions of the Zoological Society of London, in order to gather information concerning the larger mammals of the colony, and to make zoological collections, especially of the fishes.

THE following telegram has been received from Dr J. Stadling, who was sent by the Swedish Geographical Society to search for Andrée. It is dated Irkutsk, November 2:—"There being no telegraph in Siberia north of Irkutsk, I am sending this note by messenger up the River Lena to that place for telegraphic dispatch. We are now, on September 15, at the Lena delta, having been pursuing

our quest for news of Andrée since last April. We have got our row-boat ready, and in this are now starting to cross from the Lena delta to the mouth of the Olenek. If we arrive in safety we shall from thence proceed in sledges to Chatauga, the Taimyr Peninsula, and the mouth of the Yenisei in our search. The botanist who has accompanied us thus far is now returning *via* Irkutsk."

Mr N. R. Harrington and Dr Reid Hunt have returned to New York from an expedition to the Nile Valley, where they went to obtain material for the study of the life-history of *Polypterus*. The best fishing-ground was at Mansourah, forty miles from the sea. The study of this was expected to throw light on the relations of the Crossopterygian fishes to the Amphibia and the Dipnoi; but unfortunately no embryonic material was obtained. Excellent collections were, however, made for other zoological purposes.

Mr Cornelius Vanderbilt will provide funds for a botanical expedition to Porto Rico, to be undertaken by the New York Botanical Garden, under the direction of Dr N. L. Britton. The expedition left towards the end of October, and will collect for six months.

News comes from Argentina that Dr Santiago Roth, of the museum of La Plata, has discovered a series of Mesozoic mammalian remains in the Territory of Chubut. Palaeontologists will await with great interest his promised memoir on the subject.

The report of the progress of the Ordnance Survey up to March 31, 1898, has been issued as a Parliamentary Blue-Book, which is thus summarised by *The Times*. Dealing first with England and Wales, the report states that the publication of the revision of the cadastral survey on the 1-2,500 scale is proceeding as rapidly as possible, the total area published being 9,236 square miles, of which 5,217 square miles have been published during the year. The revision has now been taken up of all the counties of England and Wales which were surveyed more than twenty years ago. The publication of the revised maps on the 6in. scale has been hitherto much retarded by the publication of the new maps on this scale of London and the Tyneside towns. This latter work is, however, now approaching completion, and more rapid progress is expected in future. The total area on this scale published is 3,788 square miles, of which 3,609 square miles have been published during the year. With regard to the revision of the 1in. map, which was sanctioned by the Treasury in 1893, the report states that the field-work of the revision was begun in 1893, since when the whole of England and Wales has been revised on the ground, with the exception of a few streets in the midland counties and North Wales, which will be completed this year. Of a total area of 58,527 square miles thus revised 14,643 have been revised during the year. The revised maps of 28,305 square miles have been engraved and published, 5,282 during the year. The general result to be obtained by the revision is that in 1899 there will be available to the public for the first time a 1in. outline map of the whole of the country, prepared on one uniform system, and with its principal details nearly up to date. It is further stated that the revision of the map in the scale of 4in. to the mile will follow on that of the 1in. map, but cannot proceed very rapidly until the latter has been completed. After giving some details with regard to Scotland and Ireland, the report, in a summary and tabular statement of progress for 1897-98, shows that, so far as the original surveys of the United Kingdom are concerned, the town surveys for Great Britain and Ireland, the 1-2,500 maps for Great Britain, the 6in. maps, and the 1in. maps for Great Britain and Ireland have all been completed, while the hill engraving for the new series 1in. map of England and Wales is proceeding, 4,262 square miles out of a total of 27,569 published on March 31, 1898, having been published in 1897-98. Considerable work has also been accomplished in the way of resurveying.

CORRESPONDENCE

THE CLASSIFICATION OF BUTTERFLIES

In brief reply to your note on page 298, vol. xiii., I would state that the parity of specialisation in the feelers of the Papilionidae and Nymphalidae does not necessarily authorise the association of the groups. I have not separated the Papilionides (Parnassiidae and Papilionidae) upon any characters of specialisation, but upon a fundamental divergence in the structure of the wings, such a divergence as indicates, in my opinion, a distinct phylogenetic line and is plainly available for taxonomic purposes. The gist of my article in *Natural Science* and of that in the *Proceedings of the American Philosophical Society* is, that parallel specialisations in any one organ do not authorise association, since they have been independently acquired. Rank is a relative conception. I do not claim that the Papilionides should outrank all the other butterflies, but I do claim that they cannot be logically interpolated between any of the groups of the other butterflies, a mode of classification as old as Roesel, revived by Bates, Distant, Scudder, Reuter, and others. This interpolation cannot take place because the Papilionides possess, in vein ix. of primaries, a morphological character unshared by all the other butterflies, which again have in common a loop-like appendix to vein vii. at base (vein viii.). This appendix fades out by disintegration in certain minor groups. It is also shared by the more specialised and larger groups of the moths. The butterflies are therefore probably dichotomous, and I expect the phylogeny of the Papilionides is to be sought for in the Tineides. The two groups of diurnals have attained their character as day fliers by different routes.

My criticism of Dr Jordan's investigations is, that, valuable as they are as a contribution to the morphology of the feelers, they are valueless for taxonomy and phylogeny. I do not regard the specialisations of the veining, which takes a parallel direction so far as concerns the radius and its branches in the Lycaenidae and Pieridae, as any warrant for the association of these families. And I must naturally object to having my classification of the butterflies into two major groups, viz., Papilionides and Hesperiades, criticised upon the basis of coincidences in specialisation of certain organs common to both groups. The Papilionides possess an important part of an organ which the other butterflies want. Specialisations come in afterwards, as when I show that the Parnassians are more specialised than the Swallowtails, and that therefore we should commence our lists with *Apollo* and kindred butterflies. A. Radcliffe Grote.

Roemer Museum, Hildesheim, *7th November* 1898.

IMPORTANT NOTICE

After this number Natural Science will be published by

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